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SIXTH MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

[*From our own Correspondents.*]

SATURDAY, AUG. 20.

At the Meeting of the General Committee, a letter was read from the Marquis of Lansdowne, stating his regret at being unable to take the chair, in consequence of the alarming illness of his eldest son (the Earl of Kerry), though all his arrangements were completed for leaving London on that evening. It was then proposed that the Marquis of Northampton should be appointed Vice-President, in the room of J. Harford, Esq., who had resigned from ill health. The motion was unanimously adopted, and an express despatched to secure his Lordship's attendance on Monday evening. Some temporary arrangements were made, to facilitate the distribution of tickets, and it was agreed that the officers intrusted with this duty should be at their posts at eight o'clock on Monday morning. Sectional officers and committees were appointed, after which the General Committee adjourned to twelve o'clock on Saturday, 27th of August. To prevent the final meeting of the Association from interfering with the deliberations of the Committee as in Dublin, it was resolved, that the close of the meeting should take place on Saturday evening, in the Theatre. It was gratifying to observe, that all traces of the temporary estrangement of Sir David Brewster from his old colleagues in the Council had disappeared. The members seemed eager to welcome him, and he was equally eager to show that past differences were forgotten. The Marquis of Northampton arrived in the course of the evening.

MONDAY, AUG. 22.

At eleven o'clock, the several Sections met at their appointed stations, and proceeded to business.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

President—Rev. W. WHEWELL.
Vice Presidents—Sir D. BREWSTER, Sir W. R. HAMILTON.
Secretaries—Professor FORBES, W. S. HARRIS, Esq., F. W. JERRARD, Esq.
Committee—C. Babbage, Esq., F.R.S., F. Baily, Esq., Professor James Challis, Mr. Chatfield, Professor Mc Cullagh, Robert W. Fox, Esq., William Frend, Esq., G. Gerrard, Esq., Professor Lloyd, J. W. Lubbock, Esq., Rev. Dr. Lloyd, Provost of Trinity College, Professor Moll, Rev. G. Peacock, Professor Rigaud, Professor Ritchie, John Robison, Esq., Professor Stevelly, H. F. Talbot, Esq., Professor Wheatstone.

The President of the Section took the chair.—The Chairman stated, that according to the directions of the General Committee, the Committee of the Section had met in the morning, and set down the papers which they deemed proper to have brought before the Section on this day; giving, as usual, a preference to those subjects which had been undertaken at the suggestion or request of the Association. It was well known, he observed, that grants of money had been voted by the General Committee in aid of important researches, and some valuable discoveries had resulted; and that on that day they were about to be gratified as well as instructed, by hearing of laborious and searching observations and experiments, which, but for the valuable pecuniary aid afforded by the British Association, must have lain, as they had already done, for years, a mass of useless lumber. He concluded by stating that the Committee, in accordance with powers vested in them by the General Committee, had added to their number, the names of many highly-distinguished individuals, who had not arrived in town at the time when the list of the Committee was printed.

The President then called upon Sir David Brewster to give an account of a lens of rock salt, upon the construction of which he had been authorized by the Association assembled at Edinburgh to expend a sum of 80*l.*; as certain optical researches upon the nature and qualities of that substance, had induced men well skilled in the subject to anticipate the most favourable results to astronomy from its construction.

Sir David Brewster stated, that through the kindness and activity of Dr. Traill, he had procured from Cheshire several splendidly transparent and homogeneous crystals of rock salt: and that he had little doubt that these would in every way answer the desired end; but that, as a lens, when constructed of this material, would require to be adapted to a certain glass lens or lenses,—and as the construction of each of these and their mutual adaptation was a matter requiring not only the nicest mechanical manipulation, but also a skill and knowledge of principles which was not to be expected in workmen of an ordinary class,—he had most reluctantly been compelled to abstain from an attempt at the actual construction, but he hoped very soon to have it in his power to accomplish this most desirable object.

The President requested Mr. Baily to inform the Section, what steps had been taken towards procuring from the French observatories, the reduction of the Observations respecting certain fixed stars, for which a grant of money had been already made by the Association.

Mr. Baily replied, that as he was in daily expectation of receiving from M. Arago information respecting these reductions, which would be more precise and interesting than anything he was at present prepared to say respecting them, he trusted the Section would permit him to defer his notice of them to a future occasion.

To this request, the President, in the name of the Section, assented, and called upon Mr. Lubbock to give an account of the recent discussions of tide observations made at the ports of Liverpool and London, and for which a liberal grant of money had also been made by the Association.

Mr. Lubbock rose and stated, that through the indefatigable exertions of Mr. Dessiou, considerable progress had been made in the reduction of the observations made at Liverpool by Mr. Hutchinson.

The diurnal inequality or difference between the superior and inferior tide of the same day, which in the Thames was very inconsiderable, if not insensible, was found at Liverpool to amount to more than a foot; a matter upon which the learned gentleman laid considerable stress, as calculated to lead to important practical results. The object of these reductions was to compare the results of theory with these observations, and with those of Mr. Jones and Mr. Russell made at the port of London. The principal objects of comparison were the heights of the several tides, and the intervals between tide and tide; and these were examined in their relations to the parallax and declination of the Moon and of the Sun, and in reference to local, and what may in one sense be called accidental causes, as storms, &c. Of this latter, one of the most curious, as well as important, is the effect of the pressure of the atmospheric column. The learned gentleman stated, that M. Daussy had ascertained, that at the harbour of Brest a variation of the height of high water was found to take place, which was inversely as the rise or fall of the barometer, and that a fall of the barometer of 0.622 parts of an inch, was found to cause an increase of the height of the tide, equal to 8.78 inches in that port. To confirm this interesting and hitherto unsuspected cause of variation, had been one principal object of the researches of the learned gentleman, and at his request, Mr. Dessiou had calculated the heights and times of high water at Liverpool for the year 1784, and compared them with the heights of the barometer, as recorded by Mr. Hutchinson for the same year; and by a most careful induction, it had turned out that the height of the tide had been on an average increased by one inch for each tenth of an inch that the barometer fell, *cæteris paribus;* but the time was found not to be much, if at all, affected. Mr. Lubbock then proceeded to examine the semi-menstrual declination and parallax correction, and stated that the result was a remarkable conformity between the results of Bernouilli's theory, and the results of observations continued for nineteen years at the London Docks. But to render the accordance as exact as it was found to be capable of being, it was necessary to compare the time of the tide, not with that transit of the Moon which immediately preceded it, but with that which took place about five lunar half days. To explain this popularly, the learned gentleman stated, that however paradoxical it might appear to persons not acquainted with the subject, yet true it was, that although the tide depended essentially upon the Moon, yet, any particular tide, as it reaches London, would not be in any way sensibly affected, were the Moon at that instant, or even at its last transit, to have been annihilated; for it was the Moon as it existed fifty or sixty hours before, which caused the disturbance of the ocean, which ultimately resulted in that tide reaching the port of London. The learned gentleman then exhibited several diagrams, in which the variations of the heights of the tide, as resulting from calculations founded upon the theory, were compared with the results of observations. The general forms of the two curves which represented these two results, corresponded very remarkably; but the curve corresponding to the actual observations, appeared the more angular or broken in its form, for which the learned gentleman satisfactorily accounted, by stating that the observations were neither sufficiently numerous, nor sufficiently precise, from the very manner in which they were taken and recorded, to warrant an expectation of a closer conformity, or a more regular curvature. When it is recollected that the observations are at first written on a slate, and then transferred to the written register, by men otherwise much employed, and whose rank in life was not such as would lead us to expect scrupulous care, it was not to be wondered at, if occasionally an error of transcript should occur, or even if the observation of one transit was set down as belonging to the next. When to these circumstances it was added, that the tide at London was in all probability, if not certainly, made up of two tides, one having already come round the British Islands, meeting the other as it came up the British Channel, it was altogether surprising that the coincidence should be so exact; and it was one among many other valuable results of these investigations, that it was now pretty certain that tide tables constructed for the port of London, by the theory of Bernouilli, would give the height and interval with a precision quite sufficient for all practical purposes, and which might be relied on as sufficiently exact, when due caution was used in their construction, and the necessary and known corrections applied. In conclusion, Mr. Lubbock said, the Observations for the port of London had now been continued from the commencement of this century, and those for Liverpool, as we understood, about twenty-five years.

The arrangement of the papers made by the Committee now required the President to give a report of the proceedings of the Committee appointed by the Association to fix the relative level of land and sea; the chair was therefore taken by Sir David Brewster, one of the Vice Presidents, and Mr. Whewell presented himself to the Section.

He commenced by saying, that as in the discussion of the relative level of land and sea, the tides of the ocean were an important element, he should preface the remarks upon that subject, which he intended to submit, by making a few observations upon the very valuable communication of his friend Mr. Lubbock. This communication he highly eulogized, and pointed out to the Section the importance of many of the conclusions, should they prove hereafter to be generally applicable: but he expressed strongly his fears that this would not be the case. Observation had, in the instance of the tides, far outstripped

theory, for many reasons, which it would be impossible to detail; but among the most prominent were the complexity of the problem itself involving the astronomical theories both of the Sun and Moon; the masses of these bodies; the motions of disturbed fluids, and local causes tending to alter or modify the general geographical effect of the great tide-wave at any particular place. It was upon a careful review of these considerations, that he was led to fear that it would be still many years before theory would become so guarded and supported by local observations, as to afford a sufficiently correct guide to be implicitly relied on in these speculations. He instanced the tides of the Bristol Channel, which, in consequence of their excessive magnitude, afforded magnified representations of the phenomena by which the deviations become more remarkable. At the port of Bristol, the tide rose to a height of fifty feet, while towards the lower part of the Channel they only rose twenty, and along other parts of the coast not quite so high. The most striking of Mr. Lubbock's conclusions was that by which it appeared that the ocean assumed the form of the spheroid of equilibrium, according to the theory of Bernouilli, but at five transits of the Moon preceding the tide itself. By the calculations of Mr. Bent, however, it would appear, that although the observed laws of the tides at Bristol might be made to agree with Bernouilli's theory of equilibrium tides, by referring them to a certain anterior transit,—so far as the changes due to parallax were concerned, as also as far as those due to declination were concerned,—yet it turned out that this anterior period itself was not the same for parallax as for declination. The two series of changes have not therefore a common origin or a common epoch; so that in fact there is no anterior period which would give theoretical tides agreeing with observed tides; and, therefore, at least the Bristol tides do not at present appear to confirm the result obtained by Mr. Lubbock from the London tides. The learned gentleman then illustrated these views by diagrams, by the aid of which he explained to the Section the luni-tidal intervals, and the curve of semi-menstrual inequality—(this latter term, and the doctrine connected with it, was introduced into the subject of the tides by the learned gentleman himself, and, as is admitted by all acquainted with the subject, with the most valuable result).

Professor Whewell then proceeded to the question more immediately before him—the proceedings of the Committee appointed to fix the relative level of the land and sea, with a view to ascertain its permanence, or the contrary. He observed, that the Committee had not taken any active practical steps for the important purposes for which they were appointed, because they had met with many unexpected difficulties requiring much consideration. It was, however, intended to appoint a Committee for the same purposes, who should be furnished with instructions founded upon the views at which the former Committee had by their labours and experience arrived. One method proposed was, that marks should be made along various parts of the coast, which marks should be referred to the level of the sea; but here the inquiry met us in the very outset—what is the proper and precise notion to be attached to the phrase the *level of the sea?* Was it high water-mark or low water-mark? Was it at the level of the mean tide, which recent researches seemed to establish? In hydrographical subjects the level of the sea was taken from low water, and this, although in many respects inconvenient, could not yet be dispensed with, for many reasons, one of which he might glance at—that by its adoption, shoals, which were dry at low water, were capable of being represented upon the maps as well as the land. The second method proposed appeared to the learned Professor to be the one from which the most important and conclusive results were to be expected. It consisted in accurately levelling, by land survey, lines in various directions, and by permanently fixing, in various places, numerous marks of similar levels at the time; by the aid of these marks, at future periods, it could be ascertained whether or not the levels, in particular places, had or had not changed, and thus the question would be settled whether or not the land in particular localities was rising or falling. Still further, by running on those lines, which would have some resemblance to the isothermal lines of Humboldt, as far as the sea coast, and marking their extremities along the coast, a solution would at length be obtained to that most important practical question,—what is the proper or permanent level of the sea at a given place? Until something like this were accomplished, the learned Professor expressed his strong conviction of the hopelessness of expecting anything like accuracy in many important and even practical cases. As an example, he supposed the question to be the altitude of Dunbury Hill referred to the level of the sea: if that level of the sea were taken at Bristol, where the tide rises, as before stated, fifty feet, the level of low water would differ from the same level on the sea coast at Devonshire, where the sea rises, say eighteen feet; and supposing, as is most probable, the place of mean tide to be the true permanent level by no less a quantity than sixteen feet, which would therefore make that hill to appear sixteen feet higher, upon a hydrographical map constructed by a person taking his level from the coast of Devonshire, than it would appear upon the map of an engineer taking his level at Bristol. In the method proposed, the lines of equal level would run, suppose from Bristol to Ilfracomb in one direction, and from Bristol to Lyme Regis in the other, and by these a common standard of level would soon be obtained for the entire coast.

Professor Sir William Hamilton rose to express the sincere pleasure he felt at the masterly expositions of Mr. Lubbock and Professor Whewell. One conclusion to which Mr. Lubbock had arrived was to him peculiarly interesting, viz. that by which it appeared that the influence of the Moon upon the tides was not manifested in its effects until some time after it had been exerted, for a similar observation had recently been made by Professor Hanxeen respecting the mutual disturbances of the planets.—Mr. Lubbock rose to say, that the agreement between the results calculated from the theory of Bernouilli and those obtained from actual observation, were much more exact than Professor Whewell seemed to imagine; in truth, so close was the agreement, that they might be said absolutely to agree, since the difference was less than the errors that might be expected to occur in making and recording the observations themselves.—Mr. Whewell explained that he wished to confine his observations to the Bristol tides, as these were the observations to which he had particularly turned his attention; and, with respect to which, he should be able, at the present meeting, to exhibit diagrams to the Section, which he felt confident would amply bear out his assertions respecting these tides.—Mr. Lubbock stated, that so near, indeed so exact, had been the coincidence between the observations made at London and Liverpool, and the theory, that he was strongly inclined to believe that that coincidence would be found at length to be universal.—Professor Stevelly inquired, from Mr. Lubbock, whether he did not think it quite possible that local causes might exist, which would be fully capable of producing the deviations from the theory of Bernouilli; as, for instance, in the case of Bristol, so ably insisted upon by Professor Whewell, where the causes of the extraordinary elevation are the landlocking of the tide-wave as it ascends the narrowing channel, and the reflexions of other tide-waves from several places. Now, particularly in the case of reflex tides, may it not so happen, and does it not, in fact, happen in several places, that they bring the actual tide to a given port at a time very different from that at which the influence of the Moon and Sun, if unimpeded, would cause it to arrive, and thus separate, as Professor Whewell had stated, the origin or epoch of the variations due, suppose to parallax and declension, and even cause other deviations from Bernouilli's theory?—Mr. Lubbock replied, that unquestionably it might so happen; but, in his opinion, the discussion of a few observations, like those made at Bristol, could not be expected to point out very exactly the origin or epoch of either of the variations of parallax or declination, with sufficient exactness, to furnish secure data for determining that they did not correspond to any one common previous transit of the Moon.

Professor Whewell, who had left the room for a few moments, now returned with some diagrams, which tended to illustrate his view of the question; and, in particular, he drew the attention of the Section to the circumstance, that the diurnal inequality, which was now beginning to be observed, decided the question, inasmuch as its epoch could not by any means be attributed to the same previous transit of the Moon to which the others were referred.—Mr. Frend congratulated the meeting upon the prospect now held out of determining precisely that most important practical question, the true level of the sea.

The President then called on Mr. Lubbock for his communication respecting the formation of an empirical lunar theory.

Mr. Lubbock made some preliminary remarks, tending to prove, that although the astronomical tables connected with the lunar theory are sufficiently perfect for the more general purposes of navigation, yet astronomers are by no means satisfied with resting at this degree of exactness. It is an object much to be desired that they should reach, by calculation and theory, a degree of accuracy far beyond this, and if possible construct lunar tables in which the fixed co-efficients should be as exact as could be obtained from the use of the very best instruments, erected in fixed observatories. His object was to press the accomplishing of this by obtaining directly from observation the co-efficients and other quantities which would be necessary for the construction of tables, with as little reference to previous lunar theories as was practicable or proper. He observed, that the most important works upon the lunar theory were those of Messrs. Damoiseau and Plana. The calculations of M. Damoiseau, however, are in such a very intricate form, that it is almost impossible to verify them. The work of M. Plana constitutes an entirely new era in the lunar theory, for in it the results are developed according to the powers of excentricities, inclination, and other elements of the lunar and terrestrial orbits, as also the quantity M, denoting the ratio of the Sun's mean motion to that of the Moon. In other respects, the calculations are similar to those of Damoiseau; and in both there finally meets us the almost insuperable difficulty, that the expressions for the co-efficients are series, having a very slow conveyance, and requiring, therefore, extreme labour to deduce them with any tolerable degree of accuracy. Now, it is principally these co-efficients that Mr. Lubbock wished to have the values of deduced empirically from the best observations; and he even ventured to express a hope, that by this method tables might be constructed of such minute exactness, as might serve to check the results obtained from theory; and his anticipations were the more sanguine, because nothing was wanting to complete success but the placing of a sufficient fund at the disposal of a committee, since abundant stores of the most minutely exact observations were recorded; and persons in every way competent to their reduction were to be had.

Professor Sir William Hamilton then read his report on Mr. George B. Jerrard's mathematical researches, connected with the general solution of algebraic equations.

Sir. W. Hamilton wished, in the first place, to inform the Section, that no part of the grant of 80*l.* had been expended, which the Association had so liberally placed at his disposal for the purpose of procuring the assistance of persons competent to verify, by numerical computations, the method of Mr. Jerrard. The reason that he had not deemed it necessary to resort to this expense was, that he had, at a very early period after the meeting of the British Association in Dublin, satisfied his own mind that the method of Mr. Jerrard entirely failed in accomplishing the solution of equations of the fifth and sixth degree; and he trusted that he should be able to lay before the Section, with as much clearness as the abstruse nature of the subject would admit of, the principal steps of a demonstration, which, to the mind of the learned Professor himself, at least carried complete conviction, that the method of Mr. Jerrard was not applicable until the equation, as a minor limit, had reached the seventh degree. In order that he might carry the Section fully along with him, Professor Hamilton stated, that it would be necessary to give again a rather detailed account of the peculiarities of the very ingenious notation, devised by Mr. Jerrard, for denoting certain algebraic processes, or operations resorted to in the application of his method. The Professor then proceeded

to detail to the Section the several steps of Mr. Jerrard's method, clearly marking the steps previously known to analysts, and such as Mr. Jerrard had the merit of originating. The principal peculiarity of *formulæ* seemed to be, that in an equation, transferred in a particular manner for the purpose of eliminating the co-efficients of the original equation, the co-efficients were so ingeniously obtained as to be entirely independent of the degree of the original equation, and therefore to be of a similar form in all possible equations, the solutions of which were sought. As soon as he had prepared these formulæ, the Professor proceeded to demonstrate to the Section, that from the very nature of their connexion with the original equation, they must fail in giving its solution, where it only rose to the fourth dimension, because he showed that this would involve the solution of an equation of the sixth degree, as a preliminary step. Equations, however, of this degree had been long solved, and it was only, therefore, in connexion with the generality of Mr. Jerrard's method, that its failure, as regarded them, was of any consequence. He then proceeded to give a similar demonstration of its failure, as regarded equations of the fifth and of the sixth degree; during his discussion of this step of his demonstration, he took occasion to show, that Mr. Jerrard's method had succeeded in reducing equations of the fifth degree to tables of double entry,—a discovery, upon the value of which he enlarged considerably, and highly eulogized and complimented the author; insomuch, that he stated that if the method had accomplished nothing but this alone, Mr. Jerrard would have received the congratulations of the scientific world. He then proceeded to show, that unless the index of the equation reached as a minor limit the number seven at least, a certain intermediate equation, concerned in the elimination, would be met with, along with a multiple of it, which, therefore, would not give a number of distinct results sufficient to complete the eliminations; but, beyond that degree, he stated that he had satisfied himself, that Mr. Jerrard's method would afford solutions of equations, which, even if they should, from their complexity or other causes, be useless to the practical or merely arithmetical algebraist, yet to those engaged in prosecuting inquiries involving purely symbolic algebra, he felt confident they would afford facilities and general methods of investigation, hitherto almost unlooked for and unexpected.

Mr. Babbage complimented Sir W. Hamilton upon the very lucid exposition which he had given of a subject which he characterized as bordering upon the very extremest limits of human knowledge, and congratulated Mr. Jerrard upon the success with which he had contrived so effectually to distinguish between the symbols of operation and those of quantity, in expressing the results of elimination. Engaged, as it was well known he was, in a branch of practical numerical science, he could not suffer himself to be supposed to look with indifference upon a discovery which, if it should even fail in affording any practically important assistance to his particular branch, must yet be admitted to afford the strongest promise of advantage to the more purely abstract branch of algebraic investigation.—Professor Peacock observed that during the progress of the discussion of this question he had not failed to remark the many advantages which must result to algebra from Mr. Jerrard's method, from the collateral improvements to which the prosecution of his principal object had led, partly in suggesting new and hitherto unexplored methods of elimination, and partly by leading to a notation, which so clearly distinguished between the marks of quantity and the observations and changes which were to be resorted to in reference to them; but as to the result itself, he need characterize it no higher, when he added, that it was an advance in the science, which it did not appear that the celebrated La Grange had ever contemplated, and which was not approached by the result of Stchernhausen.—Mr. Frend regretted to differ from Mr. Babbage; but to him it appeared that this subject, instead of ranging on the very outside verge of human knowledge, on the contrary, lay at the very entrance of its portals.

Professor Phillips then read his Report of the Experiments instituted with a view to determine the Inferior Temperature of the Earth.

Mr. Phillips stated, that this subject had for a long period engaged the anxious attention of scientific men, both at home and upon the continent; that the most accurate as well as numerous experiments indicated a decided elevation of temperature as a more depressed station below the earth's surface was attained; even when the depths descended to were small, this elevation of temperature became large enough to arrest attention; in fact, the temperature of the air, of the water, of the rocks, and of the soil, was found to augment as we descend. But, in order to ascertain, if possible, what portion of this heat arose from, or was connected with, an elevated temperature of the internal parts of the globe, as well as to ascertain whether the causes of these were local or universal, and, if possible, to arrive at the law of its distribution, it was deemed a matter of much importance to get rid altogether of the effect of the air's temperature upon the thermometer, as also the action of water, because the sources of the water in mines, &c. must be in most cases entirely beyond the reach of observation. All these circumstances induced the Committee appointed by the Association to conduct experiments upon this subject, to take the temperatures of the rocks themselves alone, as the fundamental experiments. With this view, they had no less than thirty-six thermometers made and carefully compared; and, although they well knew that these thermometers, after all the care which had been bestowed upon their construction, were by no means perfect or exact, yet, as their errors had been carefully noted, by a comparison with the standard thermometers of the Royal Societies of London and Edinburgh, and each thermometer numbered, the errors admit of an easy correction. Many of these thermometers had been already placed under the care of persons adequately instructed to conduct the requisite experiments, and some of them were still in the possession of the Committee, who would gladly place them in the charge of any person giving adequate security that they should be applied to the purpose for which they had been procured. The method of using them was this: a hole large enough to receive one of the thermometers, is first drilled into the solid rock, at the bottom of the mine, pit, or other proper place of observation, to the depth of two or three feet at least; into this the thermometer is then introduced and suffered there to remain for a number of days sufficient to ensure the attainment of the temperature of the rock itself. The temperature of the air at the mouth of the pit,‡ and, if possible, the mean temperature of the place, must be observed or obtained. Professor Phillips then stated, that observations had been made in this manner, and with some of these instruments, under the directions of Professor Forbes, at mines in the Lead Hills, in Scotland, and that Professor Forbes would take some early opportunity of bringing these observations more immediately under the notice of the Section; at Newcastle, under the direction of Mr. Briddle; at Wearmouth, under the care of Mr. Anderson; near Manchester, and at Northampton, under the direction of Mr. Hodgkinson: and within a few days, Professor Phillips had been enabled, through the kindness of a friend, to place a thermometer in a deep coal mine at Bedminster, in this immediate vicinity (Bristol). The results of these observations, so far as they had as yet proceeded, amply confirmed the fact of the increase of temperature in the parts under the earth's surface. As one example, the Professor stated, that in a mine, the perpendicular depth of which, below the surface, was 525 yards, the thermometer in the rock stood at 78°, while the temperature in the open air at the mouth of the mine, varied from 30° to 80°, the mean temperature of the place being 47½°.

Prof. Forbes then gave, from memory, an account of the experiments which he had been the means of instituting in the Lead Hills. Before he did so, however, he wished to state that he had been informed that an artesian well had lately been met with in granite; and he then gave a general description of artesian wells. It was to this effect: that heretofore, in making borings in certain districts through certain alternations of clays, and at length through certain rocks, a supply of water was reached, which rapidly rose through the boring to the surface, and continued to overflow at the top, sometimes, as the term fountain indicated, in considerable quantity, and with considerable force. He instanced the artesian wells or fountains of the London clay districts; and added, that the temperature of these waters was found universally to increase with the depth of their source beneath the surface of the earth. Heretofore, no such well had been obtained by boring through the granite; and if the account, which he had received, were correct, and of its correctness he entertained little doubt, this would be a matter of considerable interest as well to the geologist as to those who were engaged in scientific pursuits similar to those now under consideration. The observations made under his directions in the Lead Hills, alluded to by Professor Phillips, were almost entirely conducted by Mr. Irvine. These observations were particularly interesting, from the fact, that the mines, in consequence of a strike among the workmen, had not been worked for many months, and at the same time it most fortunately happened that they were self-drained, that is, by machinery worked by external power, without the aid of either animals or steam. This most fortunate concurrence of favourable circumstances, which could be expected to be met with in so very few instances, at once disembarrassed the observations from many sources of error, which, but for this, would have still left considerable doubts of the results being, partially, at least, affected by the heat generated by animals residing and working in the mines, as well as of artificial fires kept up for the purposes of ventilation or of originating power. It was upon these grounds that he perceived the importance of them, but had it not been for the valuable assistance afforded him by Mr. Irvine, who descended into the mine, and placed the thermometer and made the observations, he could scarcely have been as successful as the results already obtained warranted him in hoping he should be. These results, which, of course, had not as yet reached the degree of accuracy which he still looked for, lead to the conclusion that the temperature in that mine increased about 50 of Fahrenheit for a descent of ninety-five fathoms.—Professor Stevelly stated, that as practical utility was one of the principal objects of the British Association, he might be permitted to add, that the waters of these wells, in consequence of their temperature being in general elevated above the mean temperature of the place at which they delivered their waters, had been applied to the very important practical purpose of freeing machinery of ice in winter, insomuch, that by their instrumentality, machinery, such as water-wheels, &c., which had always previously been clogged by ice for a considerable part of the winter, to the great loss of the owner's manufactory, were, by the aid of the waters of these fountains, kept constantly free, while the same water had even been previously, in some instances, conducted through the factory itself, with a view to keep up a uniform and elevated temperature within its walls, thus affording a second and a very valuable practical application.

The Rev. Mr. Craig now read a paper on Polarized Light. He stated, that he conceived the term "polarization of light" had been hastily adopted, and tended to mislead, by directing the mind to a class of phenomena, with which he thought the effects upon light were by no means to be classed. In fact, he conceived that the phenomena usually attributed to polarization, were only particular instances of the application of a general principle resulting from the very nature of light, namely, that light, when strong, could pass through the substances of several peculiarly constructed media, in a manner which, when its impulse has been previously weakened, which by several means it may be, it could by no means similarly pass. The rev. gentleman then exemplified in the common optical changes made upon the course of light, and called reflection and refraction, how rays, which before incidence had gone on together, were by these processes so separated as to produce totally separated images, and even successions of images; in this and similar cases, he conceived that the division of the rays was accompanied

‡ It appears to us that it would be most desirable that the temperature of the air at the bottom of the pit, and as close as possible to the rock in which the thermometer is placed, should be noted, for unquestionably this air cannot be excluded from the hole in which the thermometer is placed, and must have an elevated temperature.

with a weakening of the force of each part, insomuch that they would now no longer pass through media in the same way that they would have passed previously; and thus, in certain cases, resulted an inability to pass even through regular structures, such as crystals of Iceland spar, tourmaline, &c., without exhibiting phenomena arising out of the peculiarity of such structures, and which afforded diversities, as he contended, fully sufficient to account for all the phenomena usually denominated polarization. He then went in detail over five common methods of polarizing light: viz. 1. By reflections at certain angles from plates of glass; 2. By reflections from similar plates, having their under surfaces blackened, so as to absorb the rays upon their coming to the back surface of the glass, and to this glass he would refer the effects of all polished surfaces, such as varnished mahogany tables and trays, japanned metals, burnished leather, &c., and he instanced the total disappearance of all diversity of colour from varnished card of several colours, when viewed under certain circumstances, through eye-pieces of tourmaline, Iceland spar, &c. (we omit the detail of these experiments, as every one of them have been shown to be consequences of the undulatory theory of light); 3. By transmitting the ray through certain crystalline substances, such as Iceland spar; 4. By passing the ray through crystals of tourmaline cut by planes parallel to the axis of the crystal; 5. By the use of Nicholls's double fusion of Iceland spar, of the action of which upon light, the rev. gentleman took, as it appeared to us, an entirely erroneous view, for he stated that the rays which had been separated within these prisms, were reunited upon their emergence, whereas the very nature of the prism is to prevent that reunion, by causing a total reflection of the ordinary ray at the oblique common surface of the two prisms, (between which, the Canada balsam is interposed). The rev. gentleman then proceeded to explain, in connexion with his theoretic views, the play of colours observed within certain kinds of crystals of Iceland spar, the distinction between right-handed and left-handed quartz crystals, and numerous other instances derived from facts familiar to those who have studied this branch of science.

Section B.—CHEMISTRY AND MINERALOGY.

President—Rev. Professor Cumming.
Vice Presidents—Dr. Dalton, Dr. Henry.
Secretaries—Dr. Apjohn, Dr. C. Henry, W. Herapath, Esq.
Committee—Dr. Barker, Professor Daubeny, Charles T. Coathupe, Esq., Rev. W. Vernon Harcourt, Professor Hare, Professor Johnston, George Lowe, Esq., F.R.S., Professor Miller, Richard Phillips, Esq., Dr. Roget, Dr. Robert D. Thomson, Dr. Turner, Dr. T. Thompson, T. Thompson, jun., Esq., Henry Hough Watson, Esq., William West, Rev. William Whewell, Dr. Yellowley, Colonel Yorke.

Mr. Watson read a paper on the Phosphate and Pyrophosphate of Soda;—one, however, of so much detail, as scarcely to admit of compression. The results at which he arrived are the following:—1st, That phosphoric acid gives off water in being converted into the pyrophosphoric acid, and that hydrogen and carbon are component parts of the former. 2nd, That phosphoric and pyrophosphoric acids are altogether different,—different in their composition and their atomic weight; that of the phosphoric being 36.1, and that of pyrophosphoric acid, 31.7. 3rd, That the precipitate given by pyrophosphate of soda and lime water, when calcined, is black; that afforded by phosphate of soda and lime water white. 4th, That, contrary to the prevailing opinion, a solution of the pyrophosphate of soda does not spontaneously change into phosphate. [The statement contained in this paper appeared to us rather startling, and, we must add, inconclusive.]

Mr. Ettrick on a new form of Blowpipe.—The principal novelty in this apparatus was the method employed for maintaining a constant blast independent of hydrostatic pressure. This was accomplished by small bellows, thrown into very rapid action by means of a wheel and pinion, and a stop-cock inserted in the tube connecting the bellows and cylindric reservoir. To prevent the air from being too much compressed, the bellows were furnished with a valve, opening outwards, and which was pressed upon by a spring, the force of which admitted of being very readily varied. From the cylinder there are two eduction pipes, terminated by nozzles, so that by using these, and a pair of lamps, two jets of flame might be brought to bear upon the same object. He also explained, how air might be made to issue from one, and coal, or other inflammable gas, from the other; and exhibited a tube similar to that long since described by Professor Daniell, by the use of which a combustible atmosphere might be made to issue from an orifice without any previous mixture in a reservoir.

Dr. Hare, in observing on this paper, incidentally described the apparatus, which he has for many years been in the habit of using, with a view to the fusion of refractory substances; and stated, that the double tube of Daniell had been used by him many years before it was described by the person whose name it usually bears. Dr. Hare stated, that he had also used the double jet, but had long since laid it aside, from a conviction of its inutility.

Mr. Herapath then drew the attention of the Section to the composition of Bath Water, as recently determined by him, and detailed the methods of analysis which he adopted, and the results at which he arrived. This analysis, which appeared to have been elaborate and exact, was conducted in the following manner: Evaporation to dryness gave the total amount of saline matter. This was resolved into two parts by rectified spirit, which dissolved the chloride of sodium and magnesium, and left the other materials. The residue was treated with muriatic acid and alcohol, and the various matters taken up estimated in the usual way. Finally, what remained undissolved by the acidulated spirit was found to be a mixture of soda, lime, and silex, the relative quantities of which were also determined. His final results would not appear to differ materially from those of other chemists. The total amount, indeed, of saline matter obtained by him was greater, and Dr. Thomson suggested that this was probably not due to error of experiment, but to the circumstance of the waters themselves having actually altered in composition, a fact which, at least in one instance, he had established by experiment.

Dr. Hare next described his Apparatus for the Analysis, on the plan of Volta, of Gaseous Mixtures. It consists of two distinct parts, his eudiometer and calorimeter; in the former of which he measures and confines, and, by the latter of which, he fires the mixture. The combustion is not produced, as in the case of the common eudiometer, by an ordinary electric spark, but by igniting with the calorimeter a fine platinum wire, which traverses the gaseous mixture. It is unnecessary to give a more detailed account of this very ingenious apparatus, as it is figured and described in the system of Berzelius, which is, no doubt, in the hands of every chemist.

We should not, however, omit to notice a very interesting application which Dr. Hare makes of his calorimeter—namely, to the blasting of rocks. By this machine the powder can be fired at a great distance, and several trains also at the same instant,—of course, without endangering the lives of quarrymen; and, should an immediate explosion not take place upon setting the calorimeter in action, by replacing this instrument in the inactive state, which is done in an instant, the train may be approached, without fear that its ignition will ensue,—a thing which, according to the ordinary modes of blasting, can seldom be done with impunity. He also alluded to an apparatus, in which silicon and boron can be readily obtained by igniting with his calorimeter potassium enveloped by the fluosilicic or fluoboric gases, and complained that an account of this instrument, which he forwarded several years ago to the Annals of Philosophy, had been suppressed.

Mr. Herapath read a short paper on an Aurora Borealis, visible in this city on the 18th of last November, and suggested as the true theory of this very interesting, but rather perplexing, phenomenon, that it is due to the escape of electricity in streams from an excited cloud, enveloped by an atmosphere in a state of relative dryness. This view was supported by Dr. Exley and other gentlemen, who had observed them in several parts of Great Britain and Ireland, but was ably and energetically opposed by the President, Dr. Dalton, who contended, that Auroræ occur at altitudes far above the region of the clouds, and also very frequently when clouds are altogether absent. Mr. Herapath, in corroboration of his views, urged, upon the authority of Sir H. Davy, that electricity in motion through a very rarefied atmosphere would be productive of no light; but this statement was contradicted by Drs. Dalton and Hare, and would certainly appear to require confirmation, for it is inconsistent with experiments made in an artificial vacuum. In connexion with this topic of the Aurora, Dr. Hare entered into some details, tending to prove, that within a *Tornado*, such as often occurs within the Tropics, there exists a partial vacuum,—a circumstance which he attributes to an electric discharge, which passes between the earth and the space beyond our atmosphere. In both these regions, *oceans* of electric fluid are, to use a phrase of the Doctor's, accumulated; and to the discharges which took place between them, and between each, and charged clouds, he seemed to refer all the phenomena of atmospherical electricity. This hypothesis is undoubtedly novel; but, as far as it appears to us, not in very strict accordance either with facts, or the fundamental principles of electrical science. It did not excite any discussion.

Section C.—GEOLOGY AND GEOGRAPHY.

President—Rev. Dr. Buckland.
Vice Presidents—R. Griffith, Esq., G. B. Greenough, Esq., (*For Geography*) R. I. Murchison, Esq.
Secretaries—W. Sanders, Esq., S. Stutchbury, Esq., T. J. Torrie, Esq.
(*For Geography*) F. Harrison Rankin, Esq.
Committee—H. T. De la Beche, Esq., M. Van Breda, Jos. Carne, Esq., Penzance, Edward Charlesworth, Esq., Major Clerke, Lord Cole, Rev. William Conybeare, R. Griffith, Esq., Rev. William Hopkins, Robert Hutton, Esq., Boscawen Ibbotson, Esq., Rev. T. T. Lewis, James Macadam, Esq., Sir George Mackenzie, M. Van der Melen, Lord Northampton, Professor Parigot, Professor Phillips, Professor Sedgwick, William Smith, Esq., John Taylor, Esq., Dr. William West, Samuel Worsley, Esq., Rev. James Yates.

G. B. Greenough, Esq. in the chair.—A memoir was read by Mr. E. Charlesworth, being a notice of Vertebrated Animals found in the Crag of Norfolk and Suffolk. The principal object in bringing forward this subject was to establish the fact of the remains of mammiferous animals being associated with the mollusca of the tertiary beds above the London clay, in the eastern counties of England. These remains are confined to a part of the Crag formation, which appears to extend from Cromer, in Norfolk, to within a few miles of Aldborough, in Suffolk, and the depth of which was very great, wells having been sunk in it without reaching its bottom. The bones of fish, and a large portion of the testacea met with in the stratum, differ widely from those of the coralline beds, and from that part of the Crag deposit which skirts the southern coast of Essex and Suffolk. Among the mammalia, which the author states really belong to the Crag, is the *Mastodon angustidens*, of which several teeth have recently been obtained in Norfolk from localities adjoining the parish of Withingham, the spot from which D. W. Smith states the specimen to have been procured which is figured in his 'Strata Identified.' Mr. Charlesworth conceived the discovery of the remains of the mastodon in this formation, as affording an argument to prove the relative ages of rocks, as no remains of this animal have been found in America in beds more ancient than the diluvial. The remaining genera of mammiferous animals can be identified with those now existing, or with such as are found in diluvial and lacustrine deposits. The author next notices the discovery of the mineralized remains of birds, chiefly bones of the extremities of natatorial tribes, a solitary instance of a similar discovery in America being the only one recorded. He was not prepared to speak concerning the different kinds of fish, but he stated their distribution—species of Squalus being found near Orford, and what Agassiz conceives to be Platex, at Cromer. Among the most remarkable is the Carcharias Megalodon, the teeth of which are found in Suffolk, equal in size to specimens from the tertiary formations of Malta. He also alluded to the difference of the testacea in different parts of the Crag, from which he was inclined to infer there were several eras in its formation. No traces of the existence of reptilia have yet been detected, which would rather support the opinion of Dr. Beck and Deshayes, that the climate during the Crag epoch was analogous to that of the Polar regions.

Professor Sedgwick stated that he had been long aware of the existence of remains of mammalia in the Norfolk Crag, although this had been disputed by Mr. Conybeare, in his work on the Geology of England and Wales. He was rather inclined to consider the Crag as all of one epoch; and Mr. Lyell had found existing species as numerous in the lower as in the upper Crag. With regard to Mr. Charlesworth's idea of the extinction of the mastodon

in England before the formation of the diluvial beds, Professor Sedgwick conceived that was reasoning from a negative fact, and that until more extensive search had been made, no such inference could be fairly drawn. He also mentioned that remains of the beaver were found in the alluvions of Cambridgeshire, and that it might have existed in England a thousand years ago. He was confident that no cause still in existence could have produced the diluvium on the Crag; its whole appearance suggested the idea of a great rush of waters.—Mr. Conybeare was perfectly willing to correct his opinion respecting the existence of the remains of mammalia in the Crag. He was of opinion that the tertiary strata of America had not been sufficiently examined to justify the conclusion that it did not contain remains of the mastodon. He started a question—which of the species of mastodon found in other countries did the British one resemble?—Mr. Greenough mentioned, as a singular peculiarity of the diluvium of Norfolk, its containing large masses of chalk, and these have imbedded organic remains, differing in some respects from those of the chalk *in situ*. The town of Cromer seemed to be built on an immense block of chalk, contained in the diluvial formation.—Mr. Murchison dissented from Mr. Greenough's opinion. He conceived the formation of chalk was under the diluvium, and had been elevated and disrupted. He had seen at Hazeborough large platforms of chalk laid bare after a storm; near that place were needle-shaped rocks of chalk, and at Cromer the foundation of the town must rest on part of the same mass. There were strong reasons for believing that the Norfolk diluvium contained recent shells only. Mr. Lonsdale, on examination, could discover no others.—Mr. Charlesworth mentioned that Dr. Beck considered the shells of the tertiary period to be extinct species, and that at the formation of the Norfolk Crag the climate must have been very cold, like the Arctic regions. He considers the diluvial formation to have been sufficiently searched to warrant an opinion that it does not contain the remains of the mastodon. Many singular organic remains have been found there, which have been transported, as of saurians, which must have come from Yorkshire. In alluding to the fact of shells similar to those of the Crag being found at Bridlington, he was informed by Mr. Sedgwick that the formation at that place was probably part of the Crag.

A paper, by Mr. J. G. Bowman, was now read, on the Bone Caves at Cefn, in Denbighshire. A description of these has been already published in the *Philosophical Journal;* and the proprietor of the place, Mr. Lloyd, was about referring the investigation of its phenomena to Dr. Buckland. The caves are in carboniferous limestone. The roof of the lower cave is covered with stalactites, which are often broken off or blunted. The diluvium on the floor contains fragments of slate, and the upper portion animal remains in great abundance. Among these are some of a very minute size, and also elytra of beetles. A black matter is also found, with veins of reddish clay. The bones are often in fragments, the teeth are something worn; sometimes the teeth of young animals, but no indentations have been found upon them. No skulls have been discovered, nor any caprolites. The bones frequently contain gelatine, and have often manganese upon them; hair was also discovered. The stalactites seem confined to the anterior part of the cave; in the posterior part a fine sand is found.

After this, a desultory conversation took place on the exhibition of two models by Mr. Ibbotson, one of the country round Neufchâtel, in Switzerland, and the other of a part of the Under Cliff in the Isle of Wight, extending from Blackgang Chine to St. Laurence.

Mr. Greenough mentioned a new mode of engraving medals lately adopted in France, and which he conceived could be advantageously employed in laying down the varieties of surface on maps.†—Mr. Griffiths spoke of the great importance of models, like Mr. Ibbotson's, as being so well calculated to display the geological structure of a country. He suggested the importance of possessing maps, both of outline and of features, and he alluded to the magnificent map of Ireland, under the Ordnance Survey, the scale of which, being six inches to a mile, enabled the geological observer to trace the geological features with a facility before unknown. It was mentioned, that the new map of Austria was on a scale of twenty-two inches to the mile, but this Mr. Greenough considered inconveniently large. Mr. Ibbotson stated that models could be easily multiplied by employing a metal mould, and using *papier maché*, or some preparation of caoutchouc; and that they might be dissected to exhibit the internal structure, and that the materials of the strata themselves could be used as colouring matter.—Lord Northampton and M. de la Beche gave their testimony of approval.—Several gentlemen then spoke of the application of combinations of letters to geological maps, to express the more minute geological phenomena; but the general opinion was, that in geological maps simplicity should, as much as possible, be preserved, and that the best mode would be to have two maps of the same district, one without names, for the geological map, and the other with the necessary writing. Maps of this kind had been given to the Geological Society by the Archduke John of Austria.

† We shall take an early opportunity of giving some information on this interesting subject.

Section D.—Zoology and Botany.

President—Professor Henslow.
Vice Presidents—Rev. F. W. Hope, Dr. I. Richardson, Professor Royle.
Secretaries—John Curtis, Esq., Professor Don, Dr. Riley, S. Rootsey, Esq.
Committee—William Yarrell, Esq., Rev. Mr. Jenyns, T. Mackay, Esq., C. Babington, Esq., Professor Nilsson, Hon. Charles Harris, Rev. Mr. Phelps, Richard Taylor, Esq., T. C. Eyton, Esq., J. E. Bowman, Esq., W. C. Hewitson, Professor Scouler, Dr. Jacob, Rev. Mr. Ellecombe, G. J. Jeffrys, Esq., R. M. Ball, Esq., Colonel Sykes, J. L. Knapp, Esq., —— Vigors, Esq., E. Forster, Esq.

Dr. Richardson commenced the proceedings of the Section, by reading the introductory portion of his report 'On the Zoology of North America.' It did not appear probable that the progress of colonization had, as yet, extinguished any one species of animal from the country. The great similarity which existed between the animals of North America and those of Europe, as regarded their generic distinctions, connected with the dissimilarity of their species, rendered them well adapted to inquiries connected with their respective geographic distribution. Hitherto, the trivial names bestowed by the colonists upon many of those of North America, had tended to mislead naturalists. The observations, in the present report, would principally refer to the western parts of North America, including New Mexico, the Peninsula of Florida and California, down to the well-defined limits of the very different South American zoological province. Dr. Richardson then proceeded to describe the physical structure of this country, of which the Rocky Mountains formed a most remarkable feature. The altitude of many of their peaks rose above the limits of perpetual snow, and their sides were flanked by zones of different temperature, affording passages for animals from the Arctic circle to the Table Lands of Mexico, without any great alteration of climate throughout the whole extent. The temperate zones of both hemispheres might, in this way, be connected, were it not that the Cordilleras were greatly depressed at the Isthmus of Panama, and that a plain extended from sea to sea, a little further to the south. As yet, we possess no information of the elevation of the backs of these mountains, independent of the heights of some of the peaks, and the elevation of the base of the range is equally unknown. The depths of some of the tranverse valleys are considerable, and these afford passages for the migration of animals. Most of the principal rivers flowing to the east cut across the chain, and one actually rises to the west of the crests of the range. On the Atlantic side, are prairies, composing plains, gently inclining to the east, and there is an extent of land which may be likened to a long valley, which stretches from the Arctic sea to Mexico, without any transverse ridges dividing it, but merely affording three distinct water-sheds. The greatest width of the plain is about 15° of longitude, in the 40° to 50° of north latitude. This configuration gives great facility for the range of herbivorous quadrupeds from north to south, and for the migration of low-flying birds, whilst the Mackenzie furnishes a channel by which the anadromous fish of the Arctic Sea can penetrate 10° or 11° of latitude to the southward, and the Mississippi enables those of the Gulph of Mexico to ascend far to the north. The most remarkable chain east of the Mississippi, is the Alleghanies, which are about 100 miles broad, rise from a base between 1,000 and 1,200 feet, and attain an elevation from 2,000 to 3,000 feet above the sea. The strip of land between them and the coast, is 200 miles broad in the Carolinas; becomes broader still in Georgia, and sweeping round the northern extremity of the chain, joins the valley of the Mississippi. This strip influences the distribution of animal life, by extending southerly to the 5° of latitude; these forming also a barrier to the progress of anadromous fish from the Atlantic to the bottom of the Gulph of Mexico. With reference to Physical Geography, Newfoundland appears as a prolongation of the Atlantic coast line, and its zoological and botanical productions correspond to those of Labrador.

When the canals already projected shall have opened a communication between the several great inland seas which exist in North America, an interchange will take place between the fish of widely diverging waters.

The great proportion of water to land, forms a striking feature of the north-east continent. This may be zoologically divided into two districts—viz. the northern or barren grounds, and the southern or wooded. The temperature is here materially influenced by the inland sea of Hudson's Straits, and thus its capability of supporting animal life much affected. On the west of the Rocky Mountains, the northern corner appears to be similar to the eastern side or barren grounds. The general character of the country bordering the Pacific is mountainous.

With respect to the climate of North America, the eastern coast has a lower mean temperature than the western, at least in the higher latitudes. Probably the isothermal, and even the isothæral lines of the banks of the Columbia and New Caledonia correspond nearly in latitude with those of the east coast of Europe. But on the eastern side, down to the 56th parallel of latitude, the subsoil is perpetually frozen. Even in the 45th parallel, on the north side of the great Canada lakes, there is upwards of six months of continuous frost, and the grallatorial and most of the graminivorous birds can find nothing to subsist them in the winter season; and, consequently, the migration of the feathered tribes is here much more general than in the countries of Europe lying under the same parallel. The principal cause of this great difference between the climates of the eastern and western districts, may be ascribed to the configuration of the coast land, which detains the ice in its bays and gulfs, and this, in melting, materially depresses the summer heat. The decrement in the mean annual heat, corresponding to the increase in latitude, is greater in North America than in Europe, and there exists a wider difference between the temperatures of summer and winter. Dr. Richardson then concluded this introductory portion of his report, by details concerning the temperatures which had been observed at different places in the county under consideration.

A discussion then ensued, in which Mr. Rootsey, Dr. Fiske, (of America,) Rev. G. Tibbetts, and Mr. G. Webb Hall, took part, respecting the best mode of obtaining both a registry of facts and an appreciation of the causes upon which atmospheric changes depend, with a view to improve our knowledge of the laws by which climate is regulated. It was thought that a proposition might be made by this Section, recommending a scheme for instituting both local and general observations to this effect.

Mr. Rootsey exhibited a living specimen of a large spider, which he considered to be the *Aranea avicularia*, Linn., or *Mygale avicularia*, which was taken at Bristol, in a ship from the Bay of Campeachy, laden with logwood. He noticed some of the extraordinary stories which were commonly reported of this animal, such as its extremely venomous nature, &c., but which he had thus obtained an opportunity of refuting. It was also a common notion, that this species caught humming birds, by springing upon them.—Mr. Lister mentioned, that he had seen a living specimen, obtained likewise from a vessel coming from the same port, and that it fed very readily on a beetle, which was presented to it.—Mr. Hope did not consider the specimen exhibited as the true *Mygale avicularia*, but believed it to be a closely allied species described by Spix and Martins. He

possessed the true *Mygale avicularia* in his own cabinet, and had seen a specimen of this insect which was washed ashore alive in Essex, with many other exotic insects, from a wreck which occurred on that coast.

Mr. Rootsey exhibited specimens of sugar, malt, and an ardent spirit, which he had extracted from mangel wurtzel, and considered that this root might, under certain circumstances, be grown to great advantage in this country, for the purposes of manufacturing the above articles. He considered the opinion of its not being liable to injury from the attack of insects, as erroneous, and exhibited specimens of the common turnip fly, *Haltica nemorum*, which he had found feeding upon it. By the selection of particular geographical strata for its culture, the average crop might be increased from 40 or 50 tons, to 70 tons per acre. He remarked, that the refuse, after expressing the juice, appeared to be nearly or quite as nutritive to cattle as before; and that, by drying this at a peculiar temperature in the malt-kiln, a material was procured which, in smell, flavour, and other qualities, closely resembled malt; excepting that it was slightly bitter. With this malt, an excellent beer had been made. He stated the several opinions which had been formed respecting the different kinds of sugar obtained from the cane, grapes, and other plants, and had found, from numerous experiments, that the analysis of the sugar of grapes was within the average afforded by those of different cane sugars, of which he considered that there existed two distinct kinds. Upon subjecting the sugar from the mangel wurtzel to the same processes as those to which the East India sugars were submitted, he had obtained crystals in no respect different from those of the cane sugar; and he, therefore, considered the two kinds in every respect identical. He considered, that the quarter of a cwt. of the malt from mangel wurtzel was equivalent to a bushel of common malt, for the purposes of brewing. The climate best adapted to the growth of the plant, was that of the valleys rather than on the tops of hills, and from computation, he thought that there were in England about 500,000 acres of land favourable to its culture.

Mr. G. Webb Hall stated, that he had been an extensive grower of this plant, and that from experience, he was not prepared to take so sanguine a view of the benefits likely to be derived from its cultivation as Mr. Rootsey. Although crops might occasionally be grown, which yielded 60 or even 90 tons per acre, he considered, that 40 was above the average. But it was not the case, that the quantity of sugar to be obtained from any crop increased in proportion to the weight of the crop from the same ground; and in France it was found, that one crop of 20 tons would often yield as much as another of 40. In Essex, it had been found, that the plant which before Christmas yielded sugar, would after Christmas furnish only a molasses, incapable of being crystallized. The great difficulty of crystallizing the sugar, arose from the rapidity with which the acetous fermentation took place, and which in our climate it was extremely difficult to avoid. In the West Indies, the process was accomplished in three days. He considered, that sugar prepared from the mangel wurtzel in England could never compete with that from the cane; and if the manufacture of it were successful in France, it was rather to be attributed to the government regulations, by which it was protected, than to any other cause, even admitting the climate of that country to be more advantageous than that of England for its growth.

Some crystalline fragments of pure white and transparent sugar, resembling sugar candy, and of considerable dimensions, which had been naturally formed in the flowers of *Rhododendron Ponticum*, were then exhibited to the Section, by Professor Henslow. There is a minute glandular spot near the base, and on the upper surface of the ovarium, from whence exudes a thick clammy juice, which, on desiccation, crystallizes into the substance here mentioned.

Mr. G. Webb Hall read a communication 'On the Acceleration of the Growth of Wheat.' After pointing out the advantages which might accrue to agriculture, from the attention given by scientific men to certain subjects with which it was connected; and the absolute necessity which now existed for making the most extensive and careful investigations concerning many points of great importance to the success of agriculture, he proceeded to call the attention of the Section to a statement of facts, by which it would be seen that the usual period allotted to the occupation of the ground for a crop of wheat might be very materially abridged. At an average, this might be estimated at ten months, though twelve, and even thirteen, were not unusual, and eight might be considered as the shortest period for the ordinary winter wheat. By a selection of particular seed, and a choice of peculiar situation, wheat sown early in March has been, on different occasions, ripened before the middle of August, a period scarcely exceeding five months. Mr. Hall considers it an unquestionable law of vegetation that the offspring of a plant of early maturity itself seeks to become so likewise, even when placed in unpropitious circumstances, and that it recedes with reluctance from the condition of its parent. Hence the seed of a crop which has been ripened in five months has a better prospect of producing another crop equally accelerated than that from a crop which has been longer in ripening. He also asserted, that the acceleration of a crop was further promoted by thick sowing, which likewise might be considered advantageous in checking and stopping the mildew.

Dr. Richardson referred to the remark of Humboldt, that in South America the wheat crop was ripened in ninety days from the period of sowing, and stated, that about Hudson's Bay this period was only seventy days. He suggested the probable advantage that might arise from importing seed from the latter country for the purpose of furthering Mr. Hall's views; but this gentleman stated, that he had found that seed imported from a distance (and he had tried some from Italy) was liable to become diseased.—As connected with the subject of the acceleration of the growth of seeds, Professor Henslow mentioned the results of experiments which he had tried upon seeds of a species of Acacia, sent by Sir John Herschell from the Cape of Good Hope, with directions that they should be steeped in boiling water before they were sown. Some of these were kept at the boiling temperature for three, six, and fifteen minutes respectively, and had yet germinated very readily in the open border; whilst those which had not been steeped did not vegetate. It was suggested that these facts might lead to beneficial results, by showing agriculturists that they may possibly be able to steep various seeds in water sufficiently heated to destroy certain fungi or insects known to be destructive to them, without injuring the vital principle in the seed itself.—Mr. Hope mentioned a practice common in some parts of Spain, of baking corn to a certain extent, by exposing it to a temperature of 150° or upwards, for the purpose of destroying an insect by which it was liable to be attacked.—Dr. Richardson mentioned, that the seeds sold in China for the European market, were previously boiled for the purpose of destroying their vitality, as the jealousy of that people made them anxious to prevent their exportation in a state fitted for germination. Upon sowing these seeds he had, nevertheless, observed that some few of them were still capable of vegetating.

Mr. Curtis exhibited some specimens of the terminal shoots of a Pinus, which had been attacked by the *Hylurgus piniperda*, and made a few remarks upon the habits of this insect.

Dr. Daubeny communicated to the Section the partial results which he had obtained from a series of experiments he was carrying on at Oxford, respecting the effects which arsenic produces on vegetation. He was led to undertake these experiments from having received a communication from Mr. Davies Gilbert, in which he stated that there was a district in Cornwall where the soil contained a large proportion of arsenic; and that no plants could grow in it except some of the Leguminosæ. By analysis, this soil yielded him about 50 per cent. of arsenic, in the form of a sulphuret; the rest being composed principally of sulphuret of iron and a little silica. He had already ascertained that a little of the sulphuret mixed in soils produced no injurious effect on *Sinapis alba*, barley, or beans; and that they flowered and seeded freely when grown in it. Although the want of solubility in the sulphuret might be assigned as a reason for its inactivity; yet it was certainly taken up by water in small quantities, and imbibed by the roots of plants. Upon watering them with a solution of arsenious acid, he had found that they would bear it in larger proportions than was presupposed. The injurious effects of arsenious acid on vegetation in the neighbourhood of the copper-works of Bristol and Swansea was noticed by Mr. Rootsey; and Mr. Stevens mentioned the circumstance of the trout in some streams of Cornwall having been destroyed by the opening of some new mines in their neighbourhood, from which arsenical compounds were discharged, though the vegetation did not appear to be injured by them; and it was further stated, that horses were considerably injured, and rendered subject to a remarkable disease, by the effects of arsenical compounds in the same districts.

Section E.—Anatomy and Medicine.

President—Dr. Roget.
Vice Presidents—Dr. Bright, Dr. Macartney.
Secretaries—Dr. Symonds, G. D. Fripp, Esq.
Committee—Dr. O'Beirne, Dr. Bernard, Dr. James Bernard, S. D. Broughton, Esq., R. Carmichael, Esq., Dr. Carson, Bracey Clarke, Esq., E. Cock, Esq., J. W. Cusack, Esq., H. Daniel, Esq., J. B. Estlin, Esq., Dr. Evanson, W. Hetling, Esq., Dr. Hodgkin, Dr. Houston, Dr. Howell, Dr. James Johnson, R. Keate, Esq., O. King, Esq., Dr. Prichard, O. Rees, Esq., Dr. Riley, Richard Smith, Esq., J. C. Swayne, Esq., N. Vye, Esq., Dr. Yellowley.

Dr. Roget opened the business by a few words on the nature and objects of the Association, and then, for himself personally, entreated the indulgence of the members, as he had been lately suffering from a severe attack of ophthalmia. Dr. O'Beirne then read the following Report of the Dublin Committee on the Pathology of the Nervous System:—

"The Committee appointed in Dublin to investigate the Pathology of the Brain and Nervous System, feel compelled on the present occasion to confine themselves to an analysis of the cases of nervous affections, which have come under their observation, during the short period which has elapsed since they have considered themselves to be regularly appointed.

"They are of opinion that, in order to arrive at accurate Pathological conclusions on a subject so extensive and complicated, and on which the most eminent authorities are found to disagree, a very great number of cases should be first submitted to their examination—then, the symptoms of each case carefully registered, and, subsequently, accurate post-mortem examinations made, in the presence of the Committee, to ascertain the structural lesion or lesions with which the symptoms co-existed.

"As far as their investigations have yet extended, they see that the subject, if considered in all its details, will require a considerable length of time before they can accumulate such a number of cases and matured observations, as would justify them in drawing general conclusions.

"Further they have to state, that they have collected some valuable facts relating to injuries and diseases of the nerves, which seem to throw light upon the disputed points of the physiology and pathology of this portion of the nervous system. They are of opinion, however, that more extended observations on this branch of the subject, are required to be made. They would also submit the necessity of repeating those experiments on animals, upon which so many authorities rely as a foundation for their doctrines.

"The Committee, influenced by the above considerations, have decided on avoiding for the present, any attempt at drawing general conclusions. They consider it more judicious to collect and arrange for a future Report, should they be re-appointed, the abundant materials, which their opportunities enable them to supply.

"In furtherance of this object, they have been for some time engaged in registering the history and symptoms of cases of nervous affections in the wards of the House of Industry, Dublin, and the different hospitals connected with it.

"This Institution contains, independently of cases of paralysis, (estimated at about 150), the following cases of mental and nervous affections, arranged as follows:—

	Males.	Females.	
Chronic Insane	74.....	179	
Epileptic ditto.....	21.....	33	
Congenital Idiots	69.....	62	
Epileptic Idiots	14.....	20	
	178	294	Total, 472

"The number of cases which the Committee have hitherto been enabled to examine with sufficient accuracy, amounts to 41. Of these they have made an

analysis. They also have some cases of affections of individual nerves.

(Signed) James O'Beirne, M.D.
George Greene, M.D.
John Macdonnell, M.D.
R. Adams, A.M. T.C.D."

"Dublin, August 17, 1836."

Dr. O'Beirne then read a paper entitled, 'An Abstract of a Work on Tetanus,' in which he pointed out the use of tobacco enema, and dwelt at considerable length on the differences between the spurious tetanus and the true.

At the close of the paper, Dr. O'Beirne, in reply to questions put to him by some of the members, observed, that with respect to arguments deduced from the exhibition of poisons, he thought they must be drawn from analogy, and be unsatisfactory; there was no poison which produced tetanus, without producing other symptoms not peculiar to tetanus, and that all cases referred to in illustration of the subject ought to be shown in all their bearings. One gentleman in relating a case had stated, that there was tenderness of the abdomen, whereas in *real* tetanus there is no such symptom; and he believed that many others referred to as tetanus, were far from genuine.—Mr. Bracey Clarke stated, that he had observed some remarkable appearances in the bodies of horses which had died of tetanus. The intestines were always constricted, and he uniformly found on dissection, either great congestion, or positive inflammation of the lungs. Bleeding had been found to effect a cure.—A member inquired of Dr. O'Beirne, whether he had ever used oil of turpentine in this disease; he had found it beneficial as an enema; the proportion he used was ℥ij oil Tereb. to ℥j of laudanum; and pouring cold water on the head at the same time. Dr. O'Beirne had no objection to the use of the oil, but he wished not to complicate the treatment. Mr. King related a case, which terminated favourably after an immense living lumbricus had been voided.

A short description of a case of Aneurism of the Arteria Innominata, furnished by Sir D. H. Dickson, was then read.

Section F.—Statistics.

President—Sir Charles Lemon, Bart.
Vice Presidents—H. Hallam, Esq., Dr. Jerrard.
Secretaries—Rev. J. E. Bromby, C. B. Fripp, James Heywood, Esq.

Committee—J. W. Cowell, Esq., M. Dupin, Lord King, M. Von Raumer, Right Hon. T. S. Rice, Professor Babbage, Dr. Bowring, M.P., T. Wyse, M.P., Rev. E. Stanley, Col. Sykes, Dr. W. C. Taylor, Henry Woolcombe, Esq., J. Simpson, Esq., Major Clerk, — Porter, Esq., Professor Mounier, Lord Sandon, Lord Nugent, Carpenter Rowe, Esq., Thomas Moore, Esq., Rev. W. L. Bowles.

A report was read, entitled 'A few Statistical Facts, descriptive of the former and present state of Glasgow,' by James Cleland, L.L.D. This report was so comprehensive, and entered into such minute detail, both as to the past and present state of the city, that we must be content to extract the more interesting passages.—

Church Accommodation.—In conducting the government census for 1831, Dr. Cleland embraced the opportunity of ascertaining the number of sittings in the churches of the Establishment, and in the chapels of the Dissenters. The results are as follows:—

Sittings in the various places of worship in the city and suburbs, 73,425,—viz. in the Established church, 30,928; Seceders, Dissenters, Episcopalians, and Roman Catholics, 42,497; being in the proportion of only one sitting to 2 75-100th persons, or 20,291 sittings less than what is required by law, viz. church accommodation for two-thirds of examinable persons. When this statement came to be considered by the religious part of the community, efforts were made to procure additional church accommodation. A society was formed for 'erecting additional parochial churches in Glasgow and its suburbs,' and although it has been instituted little more than two years, upwards of 24,000*l.* has been subscribed, and already six churches are built, or are in course of erection. Assuming the population in 1836 to be 235,000, and that 60 sittings should be provided for every 100 of the population, it is found that although every sitting in every church of every denomination were occupied, there would be a deficiency of church accommodation to the extent of 61,594 sittings. To supply this want 61 additional churches would be required.

Roman Catholics.—Being desirous to obtain an accurate account of the number of Catholics in this city and suburbs, Dr. Cleland requested Dr. Scott, the Roman Catholic bishop, to allow him to examine his register of births and baptisms for 1830, when he found that they amounted to 915; and as he had previously ascertained that there was one birth for every 2947-100th persons in the community, he concluded that the number of Catholics in the districts referred to must be 26,965 souls.—Having a desire to know the increase in the number of Catholics in the city and suburbs during the last five years, he applied to Bishop Murdoch, the coadjutor of Bishop Scott, who also allowed the register of births and baptisms to be examined, and calculating on the same principle as in 1830, Dr. Cleland considered that the Catholics in this city and suburbs, at the end of 1835, amounted to 46,238 souls.

Trade.—The increase of trade at Glasgow, in consequence of the improvements on the river, almost exceeds belief. Less than fifty years ago, a few gabbarts, and these only about 30 or 40 tons burden, could come up to Glasgow. The recent improvements have been such, that in the year 1831, vessels drawing 13 feet 6 inches water were enabled to come up to the harbour; and now large vessels, many of them upwards of 300 tons burthen, from America, the East and West Indies, and the continent of Europe, as well as coasters, are often to be found three deep along nearly the whole length of the harbour. During the year 1834, about 27,000 vessels passed Renfrew ferry; and at some periods of the year, between 20 and 30 in one hour. A few years ago the harbour was only 730 feet long on one side; whereas it is now 3340 feet long on the north side of the river, and 1260 on the south. Till of late years there were only a few punts and ploughs for the purpose of dredging the river; now there are 4 dredging machines, with powerful steam apparatus, and 2 diving bells. Till 1834, the river and harbour dues were annually disposed of by public sale, but now they are collected by the trustees, consisting of the members of the Town Council, and five merchants appointed by them.

Amount of Revenue, Expenditure, and Debt.

Date.	Revenue.	Expenditure.	Debt.
From 1752 till 1770	£ 147 0 10	£ 2,680 4 11	£ 2,533 4 1
1835	31,910 19 3	29,609 13 11	124,003 13 9

It appears from the evidence of Mr. James Russel, harbour master for the department of steam vessels, before a Committee of the House of Commons, in May, 1836, that there were 75 steamers plying to and from Glasgow, tonnage 688,568, and that during 1835 there were 8401 arrivals of steamers, twenty of them of the largest class, and some of these about 200 feet long (equal in length to frigates of the first class).

Amount of Customs Duties collected at Glasgow, in Years ended 5th January.

Year.	Duties. £.	s.	d.	Year.	Duties. £.	s.	d.
1812	3,124	2	4½	1825	41,154	6	7
1813	7,511	6	5½	1826	78,958	13	8½
1814	7,419	12	8½	1827	71,922	8	0½
1815	8,300	4	3½	1828	74,255	0	1¾
1816	8,422	9	2½	1829	70,964	8	4
1817	8,290	18	1	1830	59,013	17	3
1818	6,802	1	3	1831	72,053	17	4
1819	8,384	3	4	1832	68,741	5	9
1820	11,000	6	9	1833	97,041	11	11
1821	11,428	19	0	1834	166,913	3	3
1822	16,147	17	7	1835	270,667	8	9
1823	22,728	17	2½	1836	314,701	10	8
1824	29,926	15	0				

It is probable, from present appearances, that the duties for 1837 will amount to 400,000*l.*

Steam Vessels.—The whole race of steam propelling projectors having left the field one by one, without being able to effect the object of their ambition, the ground was occupied by Mr. Henry Bell, who was bred a house-carpenter. Having a turn for mechanics, and a great desire to follow out what others had abandoned, he employed Messrs. John Wood & Co. of Port Glasgow, to build a boat for him, which he called the Comet, and having himself made a steam-engine of three horse power, he applied the paddles. After several experiments, the Comet plyed from Glasgow to Greenock, on the 18th January, 1812, and made five miles an hour against a head wind, while, in a short time, by simply increasing her power, she went seven miles an hour. This was the first vessel that was successfully propelled on a navigable river in Europe; and it is very remarkable, that notwithstanding the great progress in mechanical science, no improvement has yet been made on Mr. Bell's principle, although numerous efforts have been made, here and elsewhere, for that purpose. It is true, that boats go swifter now than formerly, but the propelling system remains the same.—All the new boats, either for the out sea or river trade, are of greater engine power, and are much more splendidly fitted up for the accommodation of passengers. The speed is also greatly improved. The Liverpool steam-boats, in 1831, were thought to have made good passages, when they performed the run from Liverpool to Greenock, a distance of 220 miles in twenty-four to twenty-six hours. It is now done much sooner. On Wednesday, 24th June, 1835, the City of Glasgow steam-packet left Greenock and arrived in Liverpool, in the unprecedented short period of *seventeen hours and fifty-five minutes*; and the steam-packet Manchester left the Clarence Dock, Liverpool, on Monday evening, the 15th December, 1834, and arrived in Glasgow, a distance of 240 miles, discharged and loaded her cargoes, and was back again in the same dock, within the short period of *sixty* hours.

The cabin fares of the river boats, are rather less than one penny per mile, and those of the out sea packets rather more. The fare from Glasgow to Liverpool is 1*l.* 5*s.*

Stage Coaches.—Stage coaches were first introduced into Scotland in 1678. On the 6th August, in that year, Provost Campbell and the other Magistrates of Glasgow contracted with William Hume, of Edinburgh, that he should run a coach between Edinburgh and Glasgow, a distance of 42 miles. The following is an abstract of the indenture, which is rather curious. Hume engaged with all diligence to run a coach with six able horses, to leave Edinburgh every Monday morning, and return (God willing) every Saturday night; the passengers to have the liberty of taking a cloak-bag for their clothes; the *Burgesses of Glasgow* to have a preference to the coach; the fare from the 1st March to the 1st September, to be 4*l.* 16*s.* Scots (8*s.* sterling); and during the other months, 5*l.* 8*s.* Scots. As the undertaking was arduous, and could not be gone into without assistance, the Magistrates agreed to give Hume 200 merks a-year for five years. The coach was to run for that period whether passengers applied or not, in consideration of his having actually received *two years' premium in advance*, 22*l.* 4*s.* 5½*d.* sterling.

Dr. Cleland has obtained the following curious information from Mr. Dugald Bannatyne's scrap book:—"The public have been so long familiarized with stage-coach accommodation, that they are led to think of it as having always existed. It is, however, even in England, of comparatively recent date. The late Mr. Andrew Thomson, sen. informed me that he and the late Mr. John Glassford went to London in the year 1739, and made the journey on horseback. That there was no turnpike-road till they came to Grantham, within one hundred and ten miles of London. That up to that point they travelled upon a narrow causeway, with an unmade soft road upon each side of it. That they met from time to time strings of pack horses, from 30 to 40 in a gang, by which goods seemed to have been transported from one part of the country to another. The leading horse of the gang carried a bell, to give warning to travellers coming in the opposite direction; and when they met these trains of horses, with their packs across their backs, the causeway not affording room to pass, they were obliged to make way for them, and plunge into the side road, out of which they sometimes found it difficult to get back again upon the causeway.

Intercourse with Glasgow.—Dr. Cleland has published the names and destinations of 61 stage-coaches, which arrived and departed during 313 lawful days, each averaging 12 passengers. This gave 458,232 persons in the year. By 37 steam-boats, 25 passengers each, 579,050; by the swift boats on the Forth and Clyde Navigation and Union Canal, 91,976; by the light iron boats on the Paisley Canal, 307,275; by the boats on the Monkland Canal, 31,784; and by the Glasgow and Garnkirk Railroad, 118,882. These together make the gross number of persons passing and repassing to Glasgow daily amount to 1,587,198.

Population.—The following table shows the amount of population in Glasgow and suburbs, at the time of the Reformation contrasted with the last government census in 1831:—

In 1560....4,500 1740....17,034 1831....202,426

The population fell off immediately after the restoration of Charles II.; and it is a curious fact, that it required more than half a century to make up the defalcation.

Mortality Bills.—The marriage registers in this city and suburbs, may be held as correct for all statistical purposes. The same thing applies to the register for burials; and from having been appointed to take the sole charge of conducting the enumeration and classification of the inhabitants of the city of Glasgow and suburbs, for the government census of 1821 and 1831, Dr. Cleland can vouch for their accuracy.

Bills of Mortality are understood to contain a list of Births, Marriages, and Deaths, from parochial registers, at stated periods, in connexion with the population.

Births and Baptisms.	Males.	Females.	Total.
Returns from Clergymen and Lay Pastors	3281	3116	6397
Add still-born to do.	246	225	471
Total....	3527	3341	6868
Of this number there were registered only	1678	1547	3225
Number unregistered exclusive of still-born	1603	1569	3172

The following results are derived from the census of 1830-1.

	Males.	Females.		
Births	3,527	3,341	Excess of Males	186
Under 5 years	15,422	14,855	Excess of Males	567
Under 10 years	28,549	27,435	Excess of Males	1,114
Under 15 years	39,040	38,155	Excess of Males	885
Under 20 years	47,529	50,411	Excess of Females	2,822
Under 30 years	62,706	73,419	Excess of Females	10,783
Entire population	93,724	108,702	Excess of Females	14,978
Burials	2,701	2,484	Excess of Males	217

Probability of Human Life in Glasgow, which partakes of a Manufacturing and Commercial Population.—Population, 202,425; burials, 5,185; rate of mortality, one in 39 4-100th persons.

In 1820-1, with similar machinery, the population being 147,043; burials, 3,686; the rate of mortality was one in 39 89-100th persons, or, in other words, as near as may be to the mortality of 1830-1.

In the government "Parish Register Abstract," ordered by the House of Commons to be printed, on 2nd April, 1833, it is shown, in Vol. iii. p. 496, that the rate of mortality in the metropolis, on an average of years from 1811 to 1821, was one in 39.6 persons; and the same official document shows that on an average, from 1821 to 1831, the rate of mortality was one in 39.8 persons.

In the kingdom of the Netherlands, where the registers are as correctly kept as any in Europe: population, 6,166,854; deaths, 158,800, viz. males, 81,742; females, 77,058. Rate of mortality, one in 38 82-100th persons. Births, 207,388, viz. males, 106,481; females, 100,907; there is one birth in 29 73-100th persons.

Births in Glasgow, 6,868; population, 202,426: there is one birth in 29 47-100th persons. The marriages being 1,919, there is one marriage for 105 4-100th persons. The births being 6,868, and marriages 1,919, there are 3 57-100th births to each marriage. The number of families being 41,965, there are 4 82-100th persons to each family.

Cholera.—From tables, kept by Dr. Cleland and Dr. Corkendale, it appears, that there were three eruptions of cholera, marked by the reduced number of cases. Each eruption had a period of increase, and also of gradual decrease. In the first eruption, persons poorly fed, of irregular habits, and dwelling in the crowded ill-aired parts of the city, were chiefly affected. The second eruption was more severe; the attacks were more spread over the town, and many healthy persons, and in easy circumstances, fell victims to the disease. The last eruption was milder than the second, but still surpassed the first, both in the number of cases, and in the healthy and good condition of many of the sufferers. The total number of cases 6208, is about one for every 32½ of the population. The total number of the deaths, 3005, is about one for every 67½ of the population. The progress of the disease was such, as to have seized one victim for about every *six*, and to have occasioned one death for about every *thirteen*, families.

Glasgow in 1699.—According to the report of the Municipal Corporation Commission in 1835, there were in the year 1699, 15 ships belonging to Glasgow. The foreign trade amounted to the sum of 20, 500*l.* Scots—that the merchants retailed 20 tons of French wine; 20 butts of sack; 12 butts of brandy yearly, and 1000 bolls of malt monthly. That by the decay of trade 500 houses were uninhabited, and that the rent of those inhabited had fallen nearly a third. That the best houses did not exceed the rent of 100*l.* Scots, and the worst 4*l.* Scots, except some taverns. It appears from an official document, that at that period the city of Glasgow, now the first in point of population and mercantile enterprise in Scotland, was ranked only as the fifth, and that the proportion of every 100*l.* of taxation was, for Edinburgh, 38*l.* 2*s.* 8*d.*—Dundee, 9*l.* 10*s.* 8*d.*—Perth, 7*l.* 12*s.*—Aberdeen, 7*l.* 4*s.*—Glasgow, 2*l.* 13*s.* 8*d.*

Cotton Trade.—The manufacture of linens, lawns, cambrics, and other articles of similar fabric, was introduced into Glasgow about the year 1725, and continued to be the staple manufacture till they were succeeded by muslins. On the 21st of July, 1834, Mr. Leonard Horner, one of the Parliamentary Factory Commissioners, reported to Parliament, "That in Scotland there are 134 cotton mills; that, with the exception of some large establishments at Aberdeen, and one at Stanley, near Perth, the cotton manufacture is almost entirely confined to Glasgow and the country immediately adjoining, to a distance of about 25 miles radius, and all these country mills, even including the great work at Stanley, are connected with Glasgow houses, or in the Glasgow trade. In Lanarkshire (in which Glasgow is situated) there are 74 cotton factories; in Renfrewshire, 41; Dumbartonshire, 4; Buteshire, 2; Argyleshire, 1; Perthshire, 1. In the six counties there are 123 cotton mills, nearly 100 of which belong to Glasgow."

Power Looms have increased greatly of late years—some idea may be obtained of the extent of their use in Glasgow when it is known that in 1831 four houses employed 3040 looms. These looms, on an average, weave fourteen yards each per day. Allowing each loom to work 300 days in a year, these four companies would throw off 10,101,000 yards of cloth, which, at the average price of 4½*d.* per yard, is 189,393*l.* 15*s.* per annum. The power and hand looms belonging to Glasgow in 1831 amounted to 47,127, viz. steam looms, 15,127; hand looms, in the city and suburbs, 18,537; in other towns, for Glasgow manufactures, 13,463. Since that period power looms have greatly increased.

Steam Engines.—There are in Glasgow and its suburbs 310 steam-engines, viz. 176 employed in manufactories; 59 in collieries; 7 in stone quarries; and 68 in steam boats. Average power of engines, 20.46-100th; total horses' power, 6406.

Coals.—In 1831 the quantity sent to Glasgow amounted to 561,049 tons, of which 124,000 were exported, leaving for the use of families and public works 437,049 tons.

Average Price of Coals delivered in quantities in Glasgow during Eleven Years.

	Per Ton.			Per Ton.		
	s. *d.*		*s.* *d.*		*s.* *d.*	*s.* *d.*
In 1821 ..	8 4	to	9 4	In 1827 ..	6 3	to 7 3
1822 ..	7 11	to	8 11	1828 ..	5 10	to 6 10
1823 ..	7 6	to	8 6	1829 ..	5 10	to 6 10
1824 ..	7 11	to	8 11	1830 ..	5 10	to 6 10
1825 ..	11 1	to	12 1	1835 ..	6 10	to 7 10
1826 ..	9 7	to	10 7			

Previous to the year 1755, all colliers and other persons employed in coal works were, by the common law of Scotland, in a state of slavery. They, and their wives and children, if they had assisted for a certain period at a coal work, became the property of the coal masters, and were transferable with the coal work, in the same manner as the slaves on a West Indian estate.

Timber Trade.—Messrs. Pollock, Gilmour, & Co., who are chiefly engaged in the North America timber trade, have eight different establishments, that ship annually upwards of SIX MILLIONS cubic feet of timber, to cut and collect which, and to prepare it for shipment, requires upwards of FIFTEEN THOUSAND MEN, AND SIX HUNDRED HORSES AND OXEN in constant employment; and, for the accommodation of their trade, they are owners of twenty-one large ships, the registered tonnage of which is twelve thousand and five tons, navigated by five hundred and two seamen, carrying each trip upwards of twenty thousand tons of timber at forty cubic feet per ton; all of which ships make two, and several of them three, voyages annually. It may be truly said that this establishment is unequalled in Europe.

Iron Works in Scotland in June, 1836.

Erected in or about year		Furnaces.	Tons.
1767,	Carron Company	5	9,000
1786,	Clyde	4	12,000
1786,	Wilsontown	1	3,000
1790,	Muirkirk	3	6,000
1790,	Cleland	1	2,500
1799,	Devon	3	7,000
1805,	Calder	5	15,000
1805,	Shotts	1	3,000
1825,	Monkland	3	6,000
1828,	Gartsherrie	5	15,000
1834,	Dundyvan	4	12,000
	Total	35	92,000

Exclusive of the above furnaces, there are eight additional ones in a state of forwardness—viz. two at Gartsherrie, one at Calder, one at Monkland, two at Somerlie, and two at Govan. These eight furnaces will make about 20,000 tons annually.

These works are all in the neighbourhood of Glasgow excepting five, and none of them are thirty miles distant from that city.

St. Rollox Chemical Works.—This manufactory, for the manufacture of sulphuric acid, chloride of lime, soda, and soap, the most extensive of any of the kind in Europe, covers ten acres of ground, and within its walls there are buildings which cover 27,340 square yards of ground. In the premises there are upwards of 100 furnaces, retorts, or fire places, and in one apartment there are platina vessels to the value of upwards of 8,000*l.* In this great concern, upwards of 600 tons of coal are consumed weekly.

Soft Goods Trade.—The establishment of Campbell & Co., now embracing the wholesale as well as the retail business, is the largest of the kind out of London, containing 4842 square yards of flooring, or rather more than an imperial acre. One hundred and thirty-four persons are employed in the sale and counting-house departments of these warehouses. The following is a note of the respective amounts of seven years' sales, which not only shows the progressive increase of Messrs. Campbells' business, but exhibits a fair criterion of the rapid increase and commercial improvement of the city of Glasgow:—

In 1818	£ 41,022	6	4	In 1830	£250,899	9	6
1824	156,284	2	1	1832	312,207	5	8
1827	183,385	6	10	1834	423,021	4	7

The sales during 1835 exceeded HALF A MILLION STERLING. Besides these gross sales, the company manufacture to the value of from 70,000*l.* to 80,000*l.* annually of the goods thus disposed of, giving employment from this department to nearly 2000 persons.

Post Office.—On the 17th of November, 1709, when the Magistrates of Glasgow applied to Parliament for a riding post between their city and Edinburgh, the whole Post Office revenue of Scotland was under 2,000*l.* The correspondence of Scotland seems to have been at a low ebb at the Revolution, as William III. gave a grant to Sir Robert Sinclair of Stevenson, of the whole revenue of the Post Office in Scotland, with a salary of 300*l.* per annum to keep up the post. After a fair trial, Sir Robert gave up the grant as disadvantageous.

There are four complete deliveries of letters now made daily to every part of the town and suburbs, and an answer may be received the same day to a penny-post letter put into the office, or a receiving-house, in time to be sent out with either of the two first deliveries.

Revenue at the following dates.

In 1781	£ 4,341	4	9	In 1831	£35,642	19	5
1810	27,598	6	0	1832	36,053	0	0
1815	35,784	16	0	1833	36,481	0	0½
1820	34,533	2	3	1834	37,483	3	4
1825	34,190	1	7	1835	39,954	4	6
1830	34,078	9	0½				

Markets.—Prior to 1784, when the Highland Society of Scotland was instituted, the cattle slaughtered in Glasgow were small and ill fed; since that time the quality of butcher's meat has been greatly

improved. In 1763, when deacon Peter Brown became apprentice to the butcher trade, the slaughter of bullocks was not known here; a few milch cows only were killed through the year. At Martinmas, every family that could afford it killed a small Highland cow, which was called their mart, and this served them through the greatest part of the year. Dr. Cleland has ascertained, that from the 1st of May, 1827, till 1st of May, 1828, there were 17,840 bullocks slaughtered in this city and suburbs, and 144,900 sheep and lambs. Value of butcher's meat for the above year (details published in the Annals of Glasgow), 303,978*l.* 14*s.* 5*d.*; bread, 177,266*l.* 10*s.* 8*d.*; milk, 67,342*l.* 10*s.* Total value of meat, bread, and milk, 548,587*l.* 15*s.* 1*d.* Since 1821, a great number of rumps of beef has been sent to this market yearly from Edinburgh. In 1835, there were upwards of 7,530 rumps sent here from that city, the average value of each was 20*s.* The rumps are cured as hung beef.

Live Cattle Market.—Prior to 1808, the principal butchers in this city were frequently obliged to travel a circuit of seventy or eighty miles, to purchase cattle in lots, and to rent expensive parks in the neighbourhood of the city to graze them in. The mode of supply is now completely changed. In 1808 a spacious market-place for the sale of cattle was fitted up between the south and north approach to the city in Graham Square, in which there is a commodious inn, stable, sheds, a byre to hold 120 bullocks in view, and 260 pens to contain 9,360 sheep. This market-place, which is allowed to be one of the most complete in the kingdom, occupies an area of 29,561 square yards, or rather more than six imperial acres, is paved with whin, and inclosed from the streets by ashlar stone walls. The dues, which are moderate, were let by auction at Whitsunday 1836, for a period of years, at the annual rent of 1,285*l.*, a sum which, after paying the interest of the debt, the ground-rent, repairs, and other expenses, leaves a profit to the trust fund of upwards of *five hundred pounds per annum.*

Public Executions.—From 1755 to 1830, both inclusive, 89 persons have been executed in Glasgow; of whom 5 were females. During the first 12 years only six persons were executed, while in the last 12 there were 37. During 66 years previous to 1831, there were 27 in which there were no executions, 15 in which there was one each year, 10 two, 7 three, 4 four, 1 five, and 2 in which there were six.

Shops.—In 1712 there were only 202 shops in the city, the highest rent 5*l.* and the lowest 12*l.*, average about 3*l.* In 1831 there were 3184 shops; some of them were rented at upwards of 300*l.*, the supposed average about 40*l.*

Pawnbrokers.—Prior to 1813 there were no regular pawnbrokers in Glasgow. On the 8th June, in that year, John Graham, a retired town officer, opened a pawnbroker's shop in Bell Street. In 1830 there were nineteen licensed pawnbrokers in this city; their pledges amounted to 410,400, and the capital employed in the trade to 24,700*l.*

Theatre.—It does not appear that any theatrical representation was allowed in this city from the Reformation till 1750, when Mr. Burrell, a teacher of dancing at the Bell of the Brae, gave the use of his hall for that purpose. A temporary theatre was erected against the wall of the archbishop's palace in 1752, and in a short time thereafter it was demolished by a part of a congregation who had been hearing the celebrated George Whitfield preach in the High Church Yard, who denounced it as the devil's house. At that period popular feeling against theatrical amusements was so great that dress parties were escorted to the theatre by a military guard.—In 1762 the magistrates refused to give their patronage to Messrs. Jackson, Love, and Beate, for building a theatre, and no person could be got to sell ground for that purpose. At length a theatre was erected in Grahamston, and opened in the spring of 1764, by Mrs. Bellamy and other performers. Theatrical representations continued to be so obnoxious to the people, that a mob set fire to the stage, and burned the scenery and machinery on the first night of performance. When the damage was repaired, the performances went on occasionally till 1782, when, at one o'clock in the morning of 16th April, the theatre was burned to the ground. Some time after this a small theatre was built in Dunlop Street, and opened in January, 1785; but the play-goers thinking it too small for the city, erected one in Queen Street, which was opened on 24th April, 1805, at an expense of 18,500*l.*, raised in shares of 25*l.* It was let on lease at 1200*l.*, but the lessee failed; it was then let at 800*l.*, but this could not be paid. The rent was then reduced to 400*l.*, and, ultimately, the building was sold at a price only equal to the outstanding debts and ground rent. On the forenoon of 10th January, 1829, this splendid edifice was also burned to the ground; and since that time the play-goers have contented themselves with the old theatre in Dunlop Street.

Newspapers.—The first Newspaper printed in the West of Scotland was the *Glasgow Courant*, which appeared in 1715. It was published three times a week, consisted of twelve pages in small quarto, and was sold for three halfpence, or "*one penny to regular customers.*" Since 1715 there have been 18 attempts to establish newspapers in this city, and out of that number 10 survive.

Education.—From the Reformation till 1620, there were numerous Acts of the Scotch Parliament for encouraging learning; but it would appear that these Acts had not much weight with the then Presbytery of Glasgow, as that reverend body, on 18th July, 1604, complained to the magistrates of a plurality of schools—"They thought that the Grammar School, and that taught by John Buchanan, quite sufficient." In 1816, exclusive of the University, and thirteen institutions where youths were educated, there were 144 schools; including the public institutions, there were 16,799 scholars, of whom 6,516 were taught gratis in charity or free schools. It must, however, be observed, that there were not 16,799 individual scholars, as several of them attended more schools than one. Sabbath Schools were established here in 1786. In 1820 there were 106 schools, 158 teachers, 4668 scholars, viz. boys, 2235; girls, 2433; besides three adult schools, where there were 3 teachers, and 25 male, and 54 female scholars. Of late years an improvement in the mode of education has been effected in this city, by the introduction of infant, juvenile, and normal schools; and although a number of benevolent individuals, composing the "Glasgow Educational Society," have lent their aid in accomplishing this object, every one will acknowledge that but for the unwearied and meritorious exertions of Mr. David Stow, the schools would not have been brought to their present state of perfection.

The Report concluded with an 'Abstract View of the State of Society in Glasgow at Various Periods,' from which we shall give a few extracts:—

From 1550 to 1600.—The Reformation took place during this period. The great body of the people, however, still retained their fierce and sanguinary disposition. This is strikingly marked in their being constantly armed; even their ministers were accoutred in the pulpit. The number of murders, cases of incest, and other criminal acts, which were turned over to the censures of the church, but too plainly point out the depraved character of the people.

From 1650 to 1700.—The people had become more civilized, and paid more attention to moral and religious duties. Towards the beginning of this period, nine covenanters were hanged in Glasgow, and their heads stuck on pikes on the jail. Their graves were covered with what are called the "Martyr's stones," one of which is now placed on the north façade of the Cathedral.

The Union with England opened up a spirit for trade hitherto unknown in Scotland. This great measure, which met with so much opposition in Scotland, and nowhere more so than in Glasgow, contributed greatly to the prosperity of that city.

At the time of the Union, and for half a century after it, the habits and style of living of the citizens of Glasgow were of a very moderate and frugal cast. The houses, in the early part of this century, were, almost without exception, covered with thatch, and those occupied by the highest class of citizens contained only one public room, a dining-room, and even that was used only when they had company; the family at other times usually eating in a bedroom. The people were in general religious, and, about 1745, particularly strict in their observance of the Sabbath, some of them, indeed, to an extent that was considered by others to be extravagant. There were families who did not sweep or dust their houses, did not make the beds, or allow any food to be cooked or dressed on the Sabbath. There were some who opened only as much of the shutters of their windows as would serve to enable the inmates to move up and down, or an individual to sit at the opening to read. Influenced by a regard for the Sabbath, the magistrates employed persons, termed "compurgators," to perambulate the city on the Saturday nights, and when at the approach of twelve o'clock, these inquisitors happened to hear any noisy conviviality going on, even in a private dwelling-house, they entered it and dismissed the company. Another office of these compurgators was to perambulate the streets and public walks during the time of divine service on Sunday, and to order every person they met abroad, not on necessary duty, to go home, and if they refused to obey, to take them into custody. The employment of these officials was continued till about 1750, when upon their taking Mr. Peter Blackburn, father of Mr. Blackburn of Killearn, into custody for walking on the public green on Sunday, he prosecuted the magistrates for an unwarrantable exercise of authority, and, prevailing in his suit in the court of session, the attempt to compel this observance was abandoned.

The wealth introduced into the community after the Union, gradually led to a change in the habits and style of living of the citizens. About the year 1735, several individuals built houses to be occupied solely by themselves, in place of dwelling on a floor entering by a common stair, as they had hitherto done. This change, however, proceeded very slowly, having been retarded by the effects of the rebellion of 1745, so that up to the year 1755, very few of these single houses had been built. At that period, there were only three houses from Virginia Street to Anderston, about a mile distant, excepting a few hovels, malt-kilns, and barns; now the whole line is filled up with elegant houses.

Previous to the breaking out of the American war, the Virginians who were looked up to as the Glasgow aristocracy, had a privileged walk at the cross, which they trod in long scarlet cloaks and bushy wigs; and such was the state of society, that when any of the most respectable master tradesmen of the city had occasion to speak to these tobacco lords, he was required to walk on the other side of the street, till he was fortunate enough to meet the eye of the patrician, for it would have been presumption to approach him.

Col. Sykes directed attention to the portion of the paper containing the results derived from the bills of mortality, which show that there is an excess of males from 1 to 15, but from 15 upwards an excess of females. He also remarked, that the common law of mortality, 1 in 59, must be too favourable for duration of life, since Dr. Cleland's returns and the Belgium tables gave the result only 1 in 39, and the French tables gave 1 in 44 for the northern provinces, and 1 in 40 for the southern provinces of France.—Mr. Hallam was of opinion that the average obtained in Glasgow for duration of life was not a decisive proof of inaccuracy in the general standard; because the rate of mortality must be greater in Glasgow, which was a close manufacturing town, than in agricultural districts.—Dr. Bowring said, that until the new registration bill came into full operation, the data for calculation in England must be very imperfect. He thought that the excess of females over males, above the age of fifteen, might be accounted for by the dangerous occupations in which men are engaged after that age, and also from the well-known fact, that male emigration is infinitely greater than female.—Dr. W. C. Taylor observed, that no stronger proof of the inaccuracy of the *data*, hitherto supplied to statisticians, could be given than the fact that the number of unregistered births in Glasgow, according to Dr. Cleland, very nearly equalled the number registered. He had always understood that the Scotch system of registration was superior to the English; and if those registers were actually deficient by one-half, he should be almost disposed to question the accuracy of every conclusion which has been hitherto deduced from parochial and official returns.—Mr. Fripp directed attention to the fact, that the number of Dissenters who objected to infant baptism was much greater in Scotland than in England, and that this might in some degree account for the great disparity between the registered and unregistered

births.—Col. Sykes said, that in a report embracing such a vast variety of subjects as that of Dr. Cleland, it was scarcely possible to avoid desultory discussion; he should not, therefore, apologize for passing at once to the subject of pawnbrokers, whose rapid increase in Glasgow was, he feared, no good sign of the comfort or morals of the lower classes of the population. — The colonel has suggested, said Dr. Bowring, a subject well worthy of the consideration of the Section; namely, how a series of inquiries could be framed which should procure information on topics similar to that now under discussion: in France the government had direct superintendence over the pawn-offices, and thus information in that country was attainable. He did not know how similar information could be procured in England, but he felt assured that an accurate return of the number and nature of articles pledged in pawnbrokers' shops would throw great light on the moral condition of the lower orders of society.—Mr. Fripp agreed with Dr. Bowring; and added, that he had procured from Dr. Cleland a return from the largest pawnbroking establishment in Glasgow, by which it appeared that women more frequently had recourse to this mode of raising money than men. As the subject would probably again engage the attention of the Section, he would, with the Chairman's permission, read it. (At this moment the Right Hon. Spring Rice entered the room, and was very warmly cheered.)—Mr. Fripp then read the following return of articles pledged at the largest pawnbroking establishment in Glasgow:

539 men's coats.	84 bed-ticks.
355 vests.	108 pillows.
288 pairs of trousers.	262 pairs of blankets.
84 pairs of stockings.	300 pairs of sheets.
1080 women's gowns.	162 bed-covers.
540 petticoats.	36 table-cloths.
132 wrappers.	48 umbrellas.
123 duffles.	102 bibles.
90 pelisses.	204 watches.
240 silk handkerchiefs.	216 rings.
294 shirts and shifts.	48 Waterloo medals.
60 hats.	

It was agreed that this subject should be deferred for future consideration; and most of the members intimated their intention of endeavouring to devise some plan by which more extensive information might be obtained on the subject of pawnbroking.

A long and desultory conversation followed on various topics mentioned by Dr. Cleland. The chairman recommended that the discussion of this Report should be resumed at a future day. Col. Sykes recommended the part relating to Education to the attention of the meeting, as that important subject would soon be brought under discussion by a Report from the Manchester Statistical Society, on the state of education in the borough of Liverpool.

Section G.—MECHANICAL SCIENCE.

President—Davies Gilbert, Esq.
Vice Presidents—M. I. Brunel, Esq., John Robison, Esq.
Secretaries—T. G. Bunt, Esq., G. T. Clark, Esq., William West, Esq.
Committee—Captain Chapman, G. Cubitt, Esq., J. S. Enys, Esq., William Hawkes, Esq., E. Hodgkinson, Esq., Dr. Lardner, Professor Moseley, M. Le Playe, Sir John Rennie, George Rennie, Esq., John Taylor, Esq., Rev. W. Taylor.

This Section of the Association is an off-set from the Section of Mathematics and Physics. At each of the previous Meetings it has been found necessary to appoint a Sub-Section, for the discussion of a variety of questions, having reference to the application of Physical Science in Mathematics—questions which, in the present mechanical transition state of society, have great interest and importance, and which, having specially occupied the attention of a certain class of men eminent in science, seem to claim for themselves a separate discussion. From this necessity resulted, last year, the Sub-Section, which has this year merged in Section E.

At eleven o'clock the Hall of Assembly was filled by Members of the Association, among whom we recognized some of the most distinguished of that class of individuals who, under the name of civil engineers, are working a change in the face of society, more remarkable, and, perhaps, more durable, than any which history has to record of the legislator or the conqueror—conquerors where victory is over space and time, and bloodless, and who achieve it, not by the increase of human suffering, but by the increasing profitable employment of human labour.

The discussions were opened by some observations of Professor Moseley on the theory of Locomotive Carriages.—It appeared to be the object of these remarks, to establish the importance of causing to enter into the discussion of the theory of locomotion on Rail-roads, the friction of the machinery of the locomotive carriage itself. This friction, the Professor stated to be composed of two distinct elements—one of which he termed the passive resistance of the machinery, being that which would oppose itself to a force applied to turn the wheels of the locomotive carriage if it were lifted from the rail-road. This friction he stated to amount, in some cases, to 120lb, and in others, to 170 or 180lb, in the carriages employed on the Manchester Rail-road.

Besides this passive resistance of the engine itself, independent as it is of the load, the elevation of rail-road, the velocity, or any other cause which affects the other conditions of locomotion, the Professor stated that there was a second element of the friction of the machinery, which was dependent upon, and proportional to, the load, inasmuch as it was dependent upon, and proportional to, the traction, which, in its turn, was dependent upon, and proportional to, the load.

The friction of the machinery being thus composed of two elements, one constant, and the other proportional to the load, the Professor stated that the third element of the resistance was the traction itself, being the friction of the train. The resistance being transferred, and operating upon one side of the piston, that which overcame it on the other side was the pressure of the steam, which, supposing the vaporizing power of the engine to be constant, varied inversely as the velocity. Moreover, when the machine had acquired its constant velocity, he stated that these two pressures must have become equal. These results and reasonings he stated not to be new, but not to be generally known: for the application of them to the theory of inclined planes, which followed, he claimed entire novelty. If (said the Professor) a locomotive carriage be made, with its train, to ascend an inclined plane, the following modifications will be made in the resistance to the motion of the machine and train. The passive friction of the machine will remain unchanged—the friction resulting from the traction will increase in the proportion in which the traction is increased—the traction will be increased by the resolved portion of the gravity in the direction of the plane, and it will be diminished, in so far as the friction of the train is concerned, in a ratio proportional to the course of the elevation. This diminution of friction of the train, he stated, nevertheless, to be counterbalanced by the increased distance through which the whole mass is moved; so that, on the whole, its effect was the same as though the whole had traversed a horizontal plane; so that, on the whole, the traction of the train arising from friction, and the friction of the machinery resulting from this cause, remained unaffected by the ascent. Again, the increased traction from gravity in ascent, he stated to be compensated by the diminished traction in descent, or, if the angle be greater than that of repose, by the actual acceleration in descent. But there remains, said the Professor, another element to be considered—namely, the friction of the machinery which results from that increased traction produced by *gravity* in the ascent. This he stated to have no compensation in the descent; by the ascent of the carriage the traction upon it might be increased double, the friction of its machinery would then be doubled; but, by the descent, the carriage and its train being equally accelerated, the friction resulting from traction would, indeed, be reduced to nothing, but it could assume no negative form compensating the friction of the machinery in the ascent: so that, on the whole, there was lost the whole increased friction of the machinery in the ascent, compensated by no corresponding diminution in the descent. These remarks the Professor stated to apply exclusively to the case of inclined planes having an elevation greater than the angle of repose. The loss of power in this case he stated to be considerable, amounting, perhaps, to one-fifth of the traction, and, since an inclined plane will not unfrequently double or triple the traction, amounting in the whole to a considerable fraction of the force expended and the coal consumed. The great loss of power thus resulting from the increased friction of the machinery in an ascent not compensated in the subsequent descent, the Professor stated to be an element neglected in the computations usually made on the expense of different lines of rail-road.

Dr. Lardner stated his entire concurrence in all that had fallen from Professor Moseley, and then proceeded to enter upon the discussion of a variety of facts of great interest and importance connected with the theory of rail-roads, to which, having had his attention of late called by frequent examinations before committees of parliament, he had devoted much time. The traction resulting from friction on a horizontal plane, he stated to have been estimated differently by different engineers, at from 8lb to 11lb per ton; from some experiments of his own, made with much care, he concluded it to be about 7½lb, by which sum the traction was increased by each additional ton of loading. He alluded to a variety of circumstances by which this friction may be modified, and mentioned, in particular, the effect which wetting the rail appeared to have upon it—the carriages, after a shower of rain, travelling with much greater speed and facility than before it; and he suggested, that watering pots might, with great advantage, be placed before a train of carriages, washing the rail continually for the wheels which were to follow it. The condition of the rail opposite to this, of its greatest freedom from friction, was that in which it was covered with particles of earth, triturated stone or dust; to try the effect of this condition of the rail he had strewed sand on the surface of the rail, where it had an inclination above the angle of repose, and had found the friction of this sand sufficient to counteract the tendency of the train to descend by its gravity. It having appeared, from some of Dr. Lardner's remarks, that he considered the loss of force produced by the working of a carriage over an inclined plane to result from the necessity of applying the break when the angle is greater than that of repose, and from no other theoretical cause, although *practically* there was a sacrifice in the working of the engine by reason of the superfluous steam thrown off in the descent, Professor Moseley inquired, whether, in the event of the steam thus lost being by some means husbanded from the period when the train commenced its descent, and the break not put on, he would consider any force to be lost in the whole transit of the carriage over the incline. The Doctor having declared that to be his view of the matter, Professor Moseley stated, that he had not then fully understood the bearing of his remarks, or did not agree in them. The resistance to the motion of the carriage, he again stated to be composed of a constant element, and an element varying in every case with the traction:—the constant element in working an incline, to be worked round the two sides of a triangle instead of the one side, which it would traverse if there were no incline;—and the other element, varying with the traction, and dependent upon the friction of the machinery of the engine, to be greatly increased in the ascent of the plane, and to be evanescent in the descent; thus presenting itself in the descent in no negative form, under which it might compensate the loss in the ascent.—The Section having been addressed by two of its members, whose names were unknown to us, one of whom stated the method of watering the rails to have been adopted with success on some of the tram-roads in Wales, the President rose, and shortly went over the arguments which had been used, and pointed out some of their practical relations,—mistaking, however, as it appears to us, a remark which had been made, that the effect of the friction of the *train*, as distinguished from the engine, would, on the whole, be the same, whatever be the inclination of the plane; this assertion (made by Professor Moseley) he questioned; not, it appears to us, having paid attention to the Professor's explanation, that this friction, varying in amount, as the co-sine of the inclination, and its whole effect being estimated by its amount, multiplied by the distance through which it operates, that product must, both in the ascent and the descent, vary as the product of the length of the plane by the co-sine of the inclination; that is, it must vary as the horizontal base of the plane, and be the same as though the carriage were worked along that base instead of up the plane.

The discussion here terminated, and Mr. Russell, of Edinburgh, was called upon to lay before the Section the result of certain experiments, made by

him, on the traction of boats in canals at different velocities. The researches of Mr. Russell on this subject had already occupied the attention of the Association, and are published among the reports of its last Meeting; and it is one of many instances that may be produced of the advantages which have been already conferred by it on the cause of Practical Science, that the approbation which was so largely and so justly given to Mr. Russell's communication of last year, has encouraged him to make these further investigations, of which this communication is the result.

On the general principle, of the resistance of fluids to bodies moving in them, was grounded the conclusion, that it would be an impracticable thing to move the cumbrous boats upon canals at any but very low velocities, except by an expenditure of power so great that the ordinary methods of conveyance by roads would be cheaper. It was believed, that the resistance would increase with the velocity, by a law so rapid in its variation, that for two miles an hour speed, there would be four times the resistance of one mile; for three miles, nine times that of one mile; for four, sixteen times; and so on, as the squares of the velocities. Here, there was an obstacle to rapid communication by canals, which appeared insuperable. Mr. Russell has shown that there is practically a circumstance which so completely modifies the application of this principle, that when over a certain point of speed is attained, the resistance, instead of increasing, when the speed is yet further increased, in point of fact diminishes. In one of his experiments, he found, for instance, that the resistance to the traction of a canal boat, estimated by a dynamometer, increased with the velocity of its motion nearly according to the law of the squares, up to 7½ miles per hour, being then 330lb; the speed being then increased to 8½ miles per hour, instead of further increasing the resistance, *fell* to 210lb. The speed was yet further increased, and it increased again the resistance to 236lb; yet, less, be it observed, than at 7½ miles; 12 miles an hour brought it to 352lb, scarcely more than the resistance of 7½ miles speed. These results, confirmed by a number of others, had manifestly a practical application, and they have been applied to the working of fast canal boats in Scotland. Mr. Russell has devoted himself to the explanation of them. He states, that where the water of a canal is disturbed by any cause, as, for instance, the admission of a rush of water momentarily into one extremity of it, or the impeding of a body moving in it, there is generated a certain wave, whose motion along the canal is altogether independent of the nature or velocity of the impulse given to it, and dependent only upon the depth of the canal; its velocity being precisely that which a stone would acquire in falling down one half the depth of the canal. With this velocity, the wave moves uniformly and steadily to the very end of its motion, moving slower, (if the depth of the canal remain unchanged,) but only diminishes its dimensions, until it disappears, and this not for a very considerable space. He stated, that he had himself followed waves a mile and a half, and that they had been traced unbroken for three miles from the spot where they originated.

The velocity of the wave depending on the depth of the canal, it is manifest, that each canal, differing in depth from another, will have a different velocity of wave, and that each part of the canal differing from another will alter the velocity of its wave, and thus the waves near the sides will move slower than near the centre of the canal, if the side be shallower than the middle. How, then, have these facts their application to the phenomenon observed? Thus, in the experiment described above, the velocity of the wave, ascertained by numerous experiments, was eight miles an hour. As long, then, as the boat moved at three, four, five, six, or seven miles an hour, it remained in the rear of the wave; the wave had no effect on it, as the law of the velocities was the theoretical law. At eight miles an hour the boat was, in point of fact, on the wave, and it might, indeed, be seen about the centre of the boat lifting it out of the water and diminishing the traction upon it.—(See Section A, Tuesday.)

The reading of this paper was followed by a discussion, in which Mr. Whewell, Professor Moseley, and Dr. Lardner took a part, and the meeting adjourned.

PUBLIC DINNER AT THE HORTICULTURAL ROOMS.

Nearly 500 persons were present. Dr. Lloyd, Provost of Trinity College, Dublin, in the Chair—supported on his right by the Right Hon. the Marquis of Northampton. Professor Whewell, Professor Hare (Philadelphia), Davies Gilbert, Esq., Rev. W. Vernon Harcourt, John Taylor, Esq., Treasurer of the Association, Dr. Roget, Dr. Macartney, Dr. Bowring, &c. &c. On his left, by Dr. Dalton, Lord Nugent, Professor Peacock, Rev. W. Conybeare, T. Moore, Esq., Sir D. Brewster, Professor Moseley, Dr. Lardner, Dr. Lee, Sir Charles Lemon, Bart., Professor Henslow, &c.—The usual toasts were given, and the customary speeches made; and a little after seven the company adjourned to the

GENERAL MEETING AT THE THEATRE.

Soon after the doors were opened the house was filled—gallery, pit, boxes—from the top to the bottom; and it is presumed that not less than 2000 persons must have been present. At eight o'clock the Rev. Dr. Lloyd, Provost of Trin. Coll. Dublin, took the chair, as President of last year's meeting. He soon after came forward, and thus addressed the assembled members:—

My Lords and Gentlemen,—Ever since the origin of this Association, I have looked forward to its annual meetings in the assured expectation of the highest intellectual enjoyment; and it is scarcely necessary for me to add, that in these delightful anticipations I have never been disappointed. Indeed, when I consider the purposes for which you are associated, and the powers by which those purposes are to be effected, it would seem to me impossible that any hopes of this kind, however sanguine, should end in disappointment;—for here it is my unspeakable privilege to mix with the *élite* of this great country—with all that are distinguished by talents and attainments in each of the numerous departments of science; and not more distinguished by those high qualifications, than they are by the exalted purposes for which they are met together. Those purposes are, by a more rapid and extensive communication of the lights of science as they are struck out, and by carrying these things home to the doors of all, to awaken to exertion those gigantic powers of mind, which are not confined to a few favoured spots, but which are everywhere to be found; and by establishing a more immediate and intimate communication among those engaged in kindred pursuits—to unite their exertions, as it were, into one simultaneous effort, and thereby to accelerate the progress of discovery in every line in which the mysteries of nature may be penetrated by the ingenuity and perseverance of man.

Leaving to others to seek their intellectual entertainment in the way most agreeable to their own tastes, the efforts of this Association are directed to the investigation of those realities by which we are surrounded, and of the powers with which they are invested, which, whilst they point to the being and the attributes of the One Great Source of all Existence, whom to know is to adore, do also constitute the means which He has placed within our reach, and in our hands, for the improvement of this our present condition.

This is a labour in which all of every grade are alike interested, and in which all will, at least, bid you God speed. Accordingly, it will be observed, that the regards of all, of the humble as well as of those in the most exalted stations, are directed towards your proceedings; and that everywhere multitudes continue to press around you, not merely as curious spectators, but as active workmen. Here the mechanic repairs to lay before you his inventions for giving increased effect to human industry, as well as the philosopher who seeks to render the forces belonging to inanimate matter a substitute for manual labour, and thereby to ease mankind of more than half their toils; and here also the statesman seeks to perfect himself in the knowledge of the nature and extent of the materials at his disposal, for effecting the improvements he contemplates in the social edifice.

Though myself an unprofitable spectator of your exertions, I would claim to be considered as one greatly interested in your success. I am fully sensible that this is but a poor claim to the notice with which I have been honoured; and I can assure you, gentlemen, that any language at my command would be no less poor to convey the feelings it has excited. I cannot therefore trust myself in making the attempt, but must confine myself to the simple declaration, that the feelings awakened by your unmerited kindness, far from any admixture of self-complacency, are those of the humblest, as well as of the warmest gratitude.

With respect to the Presidency itself, with which I have been so highly honoured, I think that it may be compared to a brilliant gem, to which it bears many striking analogies, but chiefly in this, that whilst it dignifies everything with which it is connected, its own native lustre can neither be impaired nor improved by any adventitious circumstance. Yet in returning this precious gem with my unfeigned acknowledgements, you will permit me to offer my hearty congratulations, that the splendid setting it is now to receive, is in so much better keeping with its own inherent beauty and its inestimable value.

At the conclusion of his address, Provost Lloyd resigned the chair to the Marquis of Northampton, who immediately came forward, and said, that he should mark his accession to sovereign power by an act of royal favour. Ladies were, by law, excluded from the platform reserved for the General Committee; but as there were many ladies greatly inconvenienced for want of seats, he would, however, without fear of the imputation of tyranny, suspend that law, and invite as many to come upon the stage as it would admit.

The Rev. Mr. Conebeare (who acted as Vice President) said he would, with much pleasure, countersign and issue the ordonnance.

The Marquis then alluded to the cause which had deprived the meeting of Lord Lansdowne's services: he was sure that there was no person present who did not feel a sympathy for the afflicted father, and a sincere anxiety for the recovery of the suffering son.* The subject was a painful one; but the illness of a young nobleman of such high promise as the Earl of Kerry would, he was assured, be deemed a grievous affliction to all who knew his merits; and secret prayers would be offered for his recovery in every heart in the assembly.

His Lordship congratulated the meeting on the great accession of members which the Association had received in Bristol. Some persons had doubted the utility of these *réunions;* but if any such sceptics were present, he would reply to them in the words of the sublimest epitaph ever written, "Monumentum si quæris, circumspice." Was it possible, when so many enlightened minds were thus brought together, —when such a blaze of light was thus kindled, that its cheering rays should not extend to other minds, and light up in other bosoms the same holy fire? The effects of such assemblages were political and moral. Here were men of every shade of denomination and opinion engaged in one united effort in the cause of science and truth—eminent men from foreign lands, united by the glorious brotherhood of mind, were here assembled, to cement the intellectual union of nations. This he regarded as a political result of the highest and most gratifying order. The moral effect of the Association arose from truth being the great object of all its labours; and every truth directly led the mind to the consideration of the Eternal Being who had given us faculties to appreciate the wonders of his creation, and the wisdom by which the universe of matter was accommodated to the universe of mind. He alluded especially to Astronomy, as leading us to reflect on the Omniscience

> That had framed such laws,
> Which but to guess a Newton made immortal.

Every true philosopher was a religious man; and he who was not religious, was *pro tanto* not a philosopher. He need not recommend the foreign members to the attention of the citizens of Bristol: the natal place of Sebastian Cabot was already too well acquainted with the advantages to be derived from commercial intercourse with distant lands. He should, however, try to enlist the ladies in the service of the Association; they already possessed great influence; he would rather see it increased than diminished; he wished that they could persuade their husbands and

* Whose death, we are sorry to have to announce, as having occurred on the very day of this address.

lovers that science was as beauteous as themselves. Seriously (said his Lordship) much is in their power: the lessons taught by maternal love cling to memory with a fond tenacity which no future instructions can ever attain: they linger there when other lessons have been effaced by worldly cares, or removed by more urgent interests: and who shall say that it was not the maternal affection pointing out the beauties of a shell, a butterfly, or a flower, that first lighted up the sparks of genius in many an infant breast, which now is shining gloriously forth, the pride and wonder of the world?

Dr. Daubeny, the Secretary, then read the following

REPORT.

Gentlemen,—The practice of the three preceding Anniversaries has prepared you to expect, at the first General Meeting that may be held, a short address, explanatory of the nature of those scientific objects which had chiefly occupied the Association on the former occasion, and, in particular, of the contents of the last published Volume of Transactions, in which the results of your labours are recorded. This it has hitherto been usual for the Local Secretaries of the year to prepare; and it seemed but a fair division of labour that such a task should, in the present instance, be allotted to the one on whom, from unavoidable circumstances, the smaller share in the other duties of the office had devolved. It was this consideration, indeed, which reconciled me to the undertaking; for had I not felt that the framing of this Address was the only part of the functions of Secretary that could be discharged at a distance from the intended place of meeting, and that the time of my colleague would be engrossed by the preparatory arrangements, in which, from my absence, I was unable till lately to participate, I should have shrunk from the responsibility of a task which involved the consideration of questions of a high and abstruse character, to several of which I feel myself but ill-qualified to do justice. It is therefore with extreme diffidence that I enter upon a task which has, at former meetings, been executed by men so eminent in science, and presume, though one of the humblest members of this great body, to exhibit to you a brief sketch of the labours of some of those individuals, whose presence amongst us sheds a lustre over our proceedings, and has contributed, more than any other circumstance, to draw together this great concourse here assembled.

There is, indeed, one circumstance, and one only, that gives me some claim to address you: I mean that of my having attended at all the meetings of this Association up to the present time, and hence having traced its progress through all its various stages, from its first small beginnings at York, up to this period of its full maturity, and having thus been enabled, by an actual participation in the business of all meetings, to form a juster estimate of the real condition of the Association, and of the services it has rendered to science, than could have been collected by the public at large.

Thus circumstanced, I have become sensible of results, flowing from the meetings of this great body, which can scarcely figure in a Report, or find expression in the accounts transmitted by the periodical press,—I have been struck by the enthusiasm elicited by the concourse of congenial minds—the friendships formed and cemented—the trains of experiment first suggested, or prosecuted anew after being long abandoned; above all, the awakening of the public mind to the just claims of Science by the celebration of these Anniversaries.

But it seems almost superfluous to dilate, to those actually present at such a meeting as this, on topics of the above description, when the mere fact of their being congregated here in such numbers, conveys the best assurance that such is already their conviction. Nor is it merely the assembling of so large a portion of the respectable inhabitants of this city and neighbourhood, nor yet the attracting from a distance so great a number of the mere amateurs of science, which justifies me in this conclusion, but it is the presence of so many hard-working, so many successful, cultivators of physical research, and their devoting to the service of the Association that most valuable of their possessions, their time, which gives me a right to assume, that the minds of those qualified to judge on such matters, are already made up respecting the beneficial influence which this Association is exerting. The volume, indeed, which now lies upon the table, and which contains the results of our last year's proceedings, not only amply sustains the former character of these Transactions, but even shows more strongly than those which have preceded it, the power which the Association has been exercising in the direct advancement of Science. It contains, in the first place, several valuable contributions to our knowledge of Magnetism,—a branch of science which, within a few years, stood in a manner isolated from the rest, but which now, thanks to the researches of living philosophers, is shown to be intimately connected with, or rather to be one of the manifestations of that mysterious, but all-pervading power, which seems to be displayed not less in those molecular attractions that bind together the elements of every compound body, than in the direction imparted to the loadstone; perhaps even in the light and heat which attend upon combustion, no less than in the awful phenomena of a thunder storm.

Considering the connexion that subsists between the sciences of Heat, Electricity, and Magnetism, and considering, likewise, the efforts made with various degrees of success, and by men of very unequal pretensions, to develope the laws of each of these sciences in accordance with mathematical formulæ, one cannot wonder that the Association should have been anxious to assign to a member, no less distinguished for the depth of his mathematical attainments, than for the range of his acquaintance with modern science, the task of drawing up a Report on the theories of these three departments of Physics, considered in relation one to the other. This, accordingly, has been executed by Mr. Whewell, whose Report stands at the commencement of the volume.

The point of view in which he was directed to contemplate the subject, possesses an interest to all who are engaged in the investigation of natural phenomena, whatever may have been the particular bent to which their researches have been directed.

All the physical sciences aspire to become in time mathematical: the summit of their ambition, and the ultimate aim of the efforts of their votaries, is to obtain their recognition as the worthy sisters of the noblest of these sciences — Physical Astronomy. But their reception into this privileged and exalted order is not a point to be lightly conceded; nor are the speculations of modern times to be admitted into this august circle, merely because their admirers have chosen to cast over them a garb, oftentimes ill-fitting and inappropriate, of mathematical symbols. To weigh the credentials of the three physical sciences which have been pointed out as mathematical, was therefore a proper office for the Association to impose upon one of its members; and I believe it will be found that no small light has been thrown upon the subject by the manner in which that trust has been discharged.

With regard, however, to Magnetism, which forms one of the subjects of Mr. Whewell's Report, much still remains to be done, before the mathematician can flatter himself that a secure foundation for his calculations has been established; and the materials for this foundation must be collected from such a variety of isolated points, distant one from the other, both in time and place, dependent for their accuracy upon the occurrence of favourable circumstances, and, after all, demanding from the observer an uncommon union of skill and experience, that there is perhaps no scientific undertaking for which the co-operation of public bodies, and even of governments, is more imperiously demanded; and the Association has, in consequence, both engaged its members in the prosecution of these researches, and has proposed to obtain for them the national assistance. To call the attention therefore of the scientific world, in a greater degree, to the present condition of our knowledge as to Terrestrial Magnetism, was the object of Captain Sabine's Report in the present volume of these Transactions; and this he has accomplished by presenting us with an elaborate abstract of the work which Professor Hansteen, of Copenhagen, had published upon that subject.

This mathematician, in the year 1811, constructed a chart, in which were laid down, so far as could be ascertained, the lines of equal variation and dip of the magnetic needle in all parts of the world. It is curious to observe the degree of coincidence which exists between these lines representing the distribution of the magnetic force, and the isothermal lines by which Humboldt has expressed the distribution of heat over the earth's surface; and this apparent connexion, the cause of which remains a mystery, is calculated to stimulate our zeal for investigating the phenomena of both. Nor is it less interesting to trace in what degree these later observations appear to confirm the general conclusions arrived at by the celebrated Halley more than a century before. That astronomer had inferred, from a general review of all that was then known with regard to the variation and dip of the needle, that there must be two magnetic axes; whilst the gradual shifting of the line of no variation from west to east, led him to propose the ingenious, though whimsical hypothesis, of a moveable globe existing in the interior of the earth we inhabit, actuated by the same forces as those which propel the hollow sphere surrounding it, and, like it, possessing a north and south magnetic pole. This interior globe, if it be supposed to move with somewhat less rapidity than the exterior shell, might, as he conceived, produce a gradual shifting of the poles from east to west, and thus account for the difference observed from time to time in the position of the magnetic axes.

Now the researches of Professor Hansteen confirm the existence of two magnetic axes, though they led him to discard the hypothesis by which Halley accounted for their progressive shifting, which, indeed, the recently-discovered connexion between Electricity and Magnetism gives us hopes of explaining more satisfactorily, as has been shown by Professor Christie in the Report read by him at our third meeting.

Since the publication, however, of the great work to which his Magnetic Chart is appended, Professor Hansteen, aware of the mystery which still overhangs the subject, has been zealously employed in attempting to remove it, by ascertaining the present state and progressive change of the magnetic forces. He has accordingly employed himself in making observations on the line of no variation, or, as he prefers to call it, the line of convergence which passes through Siberia; and, by a fortunate concurrence of circumstances, the north-western expedition lately undertaken by British navigators, has afforded the means of obtaining, at the very same time, corresponding ones on the similar line, which extends from Hudson's Bay through the United States of America. Thus the position of these lines in these two most interesting localities, has been almost simultaneously determined with an exactness before unequalled.

In conjunction with Captain Sabine, Professor Lloyd, of Dublin, has contributed, in another way, at the instance of the Association, to extend our acquaintance with the empirical laws of this interesting department of science. This they have effected by determining the dip and variation of the magnetic needle in different parts of Ireland, which it was considered the more important to ascertain, from the situation of that island in the most westerly point of Europe, at which observations could be instituted.

The distribution of the earth's magnetism through this country was determined by the above-named observers, first by a separate series of observations relating to the force of that portion of the magnetic influence which operates horizontally; secondly, by a similar series on the dip of the needle; thirdly, by means of observations both on the dip and intensity of the magnetic force made at the same time and with the same instruments.

It would occupy too much of the time of the Association, were I to attempt to point out, however briefly, the precautions adopted, and the corrections applied in order to arrive at accurate results. I shall therefore only remark, that the method by which the intensity of the magnetic force was ascertained, resembles in principle that by which philosophers determine the force of gravity. For as a pendulum when set in motion oscillates on either side of the vertical line by the force of gravity, so the needle, when drawn out of its natural position, will oscillate on either side of the magnetic meridian by the earth's magnetic force, and hence, in either case, the force may be inferred to vary, inversely, as the square of the time in which a certain number of vi-

brations are performed. In order, however, to arrive at trustworthy results, many precautions must be adopted, which are pointed out in detail in Professor Lloyd's memoir, and in particular one relating to temperature; it being found that the same needle will vary in force about 1-4000th part for every degree of Fahrenheit. Having, however, arrived at a determination of the intensity of the magnetic force at the two extremities of the Island by a sufficiently extended series of observations, namely, at Limerick by Captain Sabine, and at Dublin by Professor Lloyd, and having compared the results with those obtained by means of the same needles at a spot out of Ireland, whose magnetic intensity had been previously settled, by availing themselves of the observations of Captain James Ross, at London, our authors proceeded to estimate the relative intensity of the magnetic force at twenty-five different places within the compass of Ireland, by observations made at each of these simultaneously with others at Dublin or at Limerick. They thus obtained data by which to exhibit the law of Terrestrial Magnetism in Ireland, in a similar manner to that by which Humboldt laid down the laws of the distribution of Terrestrial Heat. The same principle was adopted in determining the lines of dip as of intensity, and the general result was obtained, that the angle which the lines of dip in Ireland make with the meridian of Dublin is 56° 48′, and that the dip increases one degree for every distance of 101 miles in a direction perpendicular to these lines.

The preceding method of estimating the intensity by the number of vibrations in a given time, only applies to that portion of the earth's magnetic force which operates in a horizontal direction. In order, therefore, to determine the whole amount of this force, observations, of the kind above alluded to, must be combined with others on the dip. This third series accordingly was instituted at twenty-three different stations in Ireland, and the result arrived at was, that the lines of absolute intensity make an angle of 33° 40′ with the meridian of Dublin, and that the intensity increases in a direction perpendicular to these lines by the 1-100th part for every 95 miles of distance.

The importance of these researches in extending our knowledge of Terrestrial Magnetism, and affording the data on which a correct theory with respect to this subject may hereafter be based, will be felt even by those who do not fully appreciate the skill and labour they required, and no better proof could be afforded of the substantial benefits arising from such an institution as the British Association, than that of having originated such an inquiry.

On the subject of Heat, Dr. Hudson, of Dublin, has detailed some experiments, the tenor of which he considers incompatible with the commonly received theory respecting its radiation, which we owe to Professor Prevost, of Geneva, inasmuch as their tendency would be to establish that cold is equally radiated with heat—a result inconsistent with the notion of the former being a negative quality. He consequently leans rather to the views of Professor Leslie, who supposed heat to be radiated in consequence of the alternate expansion and contraction of the air around, producing a series of aerial pulses.

In compliance with a wish expressed by the Meteorological Committee, Dr. Apjohn has investigated the theory of the Wet-bulb Hygrometer, and communicated an account of his experiments on this subject at the Dublin Meeting. His paper, having been already published in the Transactions of the Dublin Academy, does not appear in our Report, which, however, contains two very interesting communications on subjects of Meteorology.

Mr. Snow Harris has presented a statement of the variations of the thermometer at the Plymouth Dock-yard, as noted down by the wardens and officers of the watch, during every hour of the day and night, commencing on the 1st of May, 1832, and terminating in December, 1834, which are also checked by a concurrent series of thermometrical observations, registered every two hours, at the request of the Association, by the late lamented Mr. Harvey.

Thus have been afforded us, for two complete years, observations to contrast with those taken during 1834 and 1835, at Leith Fort, under the superintendence of the Royal Society of Edinburgh.

Mr. Snow Harris has deduced from an average of these observations the following important results:—

1st, The mean temperature of various seasons, as well as that of the entire year.

2ndly, The daily progression of temperature.

3rdly, The two periods of each day at which the mean temperature occurs.

4thly, The relation between the mean temperature of the whole twenty-four hours, and that of any single hour.

5thly, The average daily range for each month.

6thly, The form of the curves described by the march of the temperature between given periods of the day and night.

In this manner has been accomplished one of the first undertakings suggested by the British Association to its members, and promoted by its funds, and the true form of the diurnal and annual curves in an important station of our southern coast been attained, as a standard of comparison with that arrived at by Sir David Brewster in the latitude of Edinburgh, and from which they exhibit in the results some extremely curious and important discrepancies.

Professor Phillips and Mr. Gray have presented us with a continuation of those curious observations on the Quantities of Rain falling at different elevations, which had formed the subject of two preceding communications published in these Transactions.

In the first series of these, it had been shown that the difference between the quantities of rain that fell depended on two conditions—1st, the height, and, 2ndly, the temperature; the former circumstance determining the *ratio* of the difference between the two stations, and the latter its *amount*.

In the second series he showed that the ratio likewise varied at different seasons.

The present or third series presents us with a formula for expressing these variations, and points out its correspondence with the observations made.

That the quantity of rain which falls should be greater at lower than at higher elevations, is a result which, though at first sight it may appear paradoxical, is quickly perceived to harmonize with the fact, that drops of rain descend from a colder to a warmer atmosphere, and consequently condense a portion of the aqueous vapour which exists suspended in the lower strata. But that the rate of increase should actually be found reducible so nearly to a mathematical formula, is certainly far more than could have been expected, and its successful accomplishment is calculated to give us hopes that other meteorological phenomena, which seem at present so capricious as to baffle all calculation, may at length be found reducible to certain fixed principles. So far as relates to the rain that falls at York, the results are regarded by Professor Phillips as sufficiently complete, but he strongly urges the advantage of instituting in other spots selected in different parts of the kingdom similar observations, which, if executed simultaneously, would mutually illustrate each other, and be likely to throw much additional light on the theory of rain, and on the distribution of vapour at different heights.

An important practical paper has been published in our Transactions of this year by Mr. Eaton Hodgkinson, on the effect of impact upon beams. It is a continuation of some researches which he communicated at the preceding Meeting, on the collision of imperfectly elastic bodies. In these experiments he had laid down the general principles relating to the collision of bodies of different natures, and had obtained, amongst other results, the following,—namely, that all rigid bodies possess some degree of elasticity, and that amongst bodies of the same class the hardest are generally the most elastic.

It remained to be seen whether this difference in elasticity influenced the force of their impact, and this he has shown in his present memoir not to be the case, the hardest and most elastic substances producing no more effect upon a beam, than any soft inelastic body of equal weight. Various other conclusions of much practical as well as theoretical importance are stated in the above paper, and the results are severally borne out by an elaborate and careful series of experiments.

Our Foreign Associate, Mons. Quetelet, has presented to us a sketch of the progress and actual state of the Mathematical and Physical Sciences in Belgium, of interest, not only from the information it conveys, but likewise as the contribution of a distinguished foreigner, who had evinced already his respect for this Association, by attending one of its meetings. The appearance of this Report, together with that published in the preceding volume by Professor Rogers, of Philadelphia, on the Geology of North America, I regard as a new proof of our prosperity. It shows that the Association has begun to exert an influence over the progress of Science, extending even beyond the sphere which by its name of British, it claims for its own, and that it has enlisted in its behalf the sympathies, not only of our Transatlantic brethren, who speak the same language, and boast of a common extraction, but likewise of those Continental nations, from whom we had so long been severed.

On the subject of Chemistry, our transactions of this year contain only a short report by Dr. Turner, explanatory of the sentiments of the members of the Committee which had been appointed the preceding year, to consider whether or not it would be possible to recommend some uniform system of Notation, which, coming forward under the sanction of the most distinguished British chemists, might obtain universal recognition. In the discussion which took place when this subject was brought before us at Dublin, three systems of Notation were proposed, differing one from the other, no less in principle, than in the end proposed by their adoption;—the first was that suggested by the venerable founder of the Atomic Theory, Dr. Dalton, who aimed at expressing by his mode of notation, not merely the number of atoms of each ingredient which unite to form a given compound, but likewise the very mode of their union, the supposed collocation of the different atoms respectively one to the other. He proposed, therefore, a sort of pictorial representation of each compound which he specified, just as in the infancy of writing, each substance was indicated, not by an arbitrary character, but by a sign bearing some remote resemblance to the object itself. This, therefore, may be denominated the Hieroglyphical mode of Chemical Notation; it was of great use in the infancy of the Atomic Theory, in familiarizing the minds of men of science to the mode in which combinations take place, and thus paved a more ready way to the reception of this important doctrine. Even now, it may have its advantages in conveying to the mind of a learner, a clearer notion of the number and relation of the elements of a compound body one to the other; and in those which consist only of two or three elements; a symbolic representation after Dr. Dalton's plan might be nearly as concise as any other. But it would be difficult, consistently with brevity, to express in this manner any of those more complicated combinations that meet us in every stage of modern chemical inquiry, as for instance, in the compounds of Cyanogen, or in proximate principles of organic life.

The second mode of Notation is that in which the method adopted in Algebra is applied to meet the purposes of Chemistry. This method, whilst it is recommended by its greater perspicuity, and by its being intelligible to all educated persons, has the advantage also of involving no hypothesis, and being equally available by persons who may have taken up the most opposite views of the collocation of the several atoms, or who dismiss the question as altogether foreign to their consideration. This, therefore, may be compared to the alphabetical mode of writing in use amongst all civilized nations; the characters indeed may differ, the words formed by a combination of these characters may be very various, but the principles on which they are put together to express certain sounds and ideas are in all countries the same.

The third method of Notation, which has been recommended by the authority of several great Continental chemists, and especially of Berzelius, resembles rather a system of shorthand than one of ordinary writing; its express object being to abbreviate, so far as is consistent with perspicuity, the mode of Notation last described. But although most chemists may find it convenient to employ some of these abbreviated forms of expression, it seems doubtful whether any particular amount of them can be recommended for general adoption, since the necessity for it will

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vary according to the habits of the individual, the nature of his inquiries, and the objects for which his notes are designed.

A chemist, for example, the character of whose mind enables him quickly to perceive, and clearly to recollect minute distinctions, may find a much more abbreviated style of notation convenient, than would be at all advisable to others; one who is engaged in the analysis of organic compounds will be more sensible of the utility of such symbols, than another who is conversant chiefly with a less complicated class of combinations; and one who notes down the results of his experiments for the benefit of private reference, and not with any immediate view to others, may indulge in a more concise and complex system of notation, than would be convenient, where either of the latter objects were contemplated.

As the shortest road is proverbially not always the most expeditious, so in Chemical Notation more time may often be lost in correcting our own blunders and those of the compositor, where dots and commas of many sorts are introduced in the place of initial letters to express certain elements, than was gained by the more compendious method of expression employed. Add to which, in the preference given to one set of dots over another, or in the particular collocation of them, above, below, or at the side of, the symbol to which they are referred, we have no fixed principle to guide us, and can therefore only be determined by the greater or less frequent adoption of one method than of another.

Perhaps, therefore, all that can be hoped from a Committee of British Chemists would be, to set forward the various uses of some system of Chemical Notation, the purposes for which each of those brought before them seems chiefly applicable, and the degree of prevalence which one has obtained over the rest.

If I may be allowed to offer my own humble opinion on a point which has been so much debated amongst British chemists, I should remark that for the purpose of rendering more intelligible to beginners the mode in which various bodies are supposed to combine, the Daltonian method of Notation may still be of use, just as pictorial representation often comes in aid of verbal description to convey the idea of a complex object. But that where the design is to state in the clearest, and least hypothetical terms, the nature of a series of combinations, a mode of notation as closely as possible approaching to that adopted in algebra seems preferable—remembering always, that as in algebra we omit certain signs for the sake of greater brevity, so it may be allowable to do in applying its principles to Chemistry, these abbreviations being of course the most advisable in cases where, by reason of the greater number of elements involved, the expression of them at whole length would occupy so much space as to prevent the whole from being comprehended at a glance.

The above remarks will not, I believe, be found inconsistent with the spirit of the brief report which Dr. Turner has communicated, and which is to the following effect:—

1st. That the majority of the Committee concur in approving of the employment of that system of Notation which is already in general use on the Continent, though there exist among them some difference of opinion on points of detail.

2ndly. That they think it desirable not to deviate in the manner of notation from algebraic usage, except so far as convenience requires.

And 3dly. That it would save much confusion if every chemist would state explicitly the exact quantities which he intends to represent by his symbols.

But I must hasten on to those few other Reports which the present volume contains, but on which I shall have the less to say, as they relate to subjects connected with Anatomy and Physiology, of less general interest to a mixed audience.

Dr. Jacob has replied to a query proposed by the Zoological Committee at a former meeting with respect to the uses of the infra-orbital cavities in Deers and Antelopes, and has pronounced them to be designed as the receptacles of a peculiar odoriferous secretion.

Dr. Hodgkin and Dr. Roupell have detailed a series of experiments and observations relative to the specific mode of action of acrid poisons, which, whether at once introduced into the stomach, or the circulation, by injection into the veins, seem to operate primarily in the same manner as irritants to the mucous membrane. The Dublin Sub-committee appointed for the purpose, have given in a report connected with a subject of great pathological interest, respecting which none but the experienced medical practitioner ought to pretend to pass a decided judgment;—nevertheless, when I look back to the early period of my own professional studies, and recollect the obscurity in which diseases of the heart appeared then to be involved, when their remedy seemed so desperate, as to suggest to one of the most distinguished writers on the subject the motto '*Hæret lateri lethalis arundo*' as appropriate to his work and as significant of the probabilities of cure, and when their very nature was known but partially, and could only be guessed at by methods purely empirical,—when I recollect all this, I cannot refrain from congratulating those of my brethren who are engaged in the duties of the profession from which I am myself a *deserter*, on the discovery of a new instrument of investigation in diseases of this nature, the use of which being founded on physiological principles, seems susceptible of greater improvement and more extended application in proportion as our knowledge of the animal economy advances.

But in order properly to avail ourselves of the indications of disease afforded by the differences of sound transmitted through the integuments by the heart, it is necessary that we should be acquainted with the nature of its pulsations, and of the sounds occasioned by them in a healthy state, and this information it has been the object of the Dublin Sub-committee to embody in the report which was communicated by them last year to the Medical Section.

Such are the principal contents of the volume which records the scientific labours instituted at the express suggestion of the general body, and prepared for its last Meeting; but, exclusively of these, many very valuable and elaborate investigations were submitted to the several Sections without any such solicitation.

I may instance in particular the views with respect to the classification and the geological distribution of Fishes, expounded to us with so much ability by Mons. Agassiz, whose important labours might perhaps have been suspended, but for the timely assistance dealt out to him by this body, and the opportunities which its Meetings afforded, for giving them that publicity which they deserved.

I may point out likewise the important results submitted to the Geological Section by Mr. Murchison and Professor Sedgwick, with reference to the Silurian formations of Wales and Shropshire, and the multitude of facts illustrative of the physical structure of Ireland, which were elicited by the exhibition of Mr. Griffith's Geological Map, an undertaking which, coupled with the researches of Mr. Mackay on the plants indigenous to that country, promises to render us as well acquainted with the Natural History of this portion of the Empire, as we already are with respect to Great Britain itself.

Nor must I forget the researches on Comparative Anatomy laid before the Medical Section by Dr. Houston, who pointed out the existence of reservoirs connected with the veins leading to the lungs in the Cetacea, an admirable contrivance, by which Nature has provided for the unobstructed circulation of their blood, in spite of the enormous pressure which they have to sustain at the great depths to which they are wont to dive.

The Members of the Association had also the satisfaction of witnessing the ingenious manner in which Mr. Snow Harris contrives to render quantities of Electricity appreciable by the balance, like those of any gross material substance; whilst such as could enter upon the more refined branches of mathematical analysis must have listened with profound interest to the exposition given by Professor Hamilton, of the ingenious labours of Mr. Jerrard, of this city, in solving Equations of the higher orders.

What proportion of such inquiries may be attributable to the influence of this Association, and how much might have been merely the result of that increased taste for physical research to which the Association itself owes its existence, I do not pretend to determine; this, however, at least, must be allowed, that many of the most important truths communicated, might have been long in winning their way to general recognition, and in ridding themselves of those exaggerated and mistaken views which are the common accompaniments of every infant discovery, had it not been for the opportunities which these Meetings afford, of examining the very authors of them, with respect to their own inquiries; of confronting them with others who have prosecuted similar trains of research; of questioning them with respect to the more doubtful and difficult points involved; and of obtaining from them, in many instances, an exhibition of the very experiments by which they had been led to their conclusions. And it is this personal intercourse with the authors of these great revolutions in Science, which in itself constitutes one of the principal charms of these meetings. Who would not have listened with delight to a Newton, had he condescended to converse on the great truths of Astronomy; to a Jussieu, imparting to a circle of his intimates in his own garden at Trianon, those glimpses with respect to the natural relations of plants, which he found it so difficult to reduce to writing; or to a Linnæus, discussing at Oxford his then novel views with respect to the vegetable kingdom, and winning from the reluctant Dillenius a tardy acknowledgment of their merits? And in like manner, who does not value the privilege of hearing a Dalton discourse on these occasions on his own Atomic Theory, or a Faraday, (who, however, I regret to say, is on this occasion prevented by illness from attending), explain orally the steps by which he has traced the relations between Electricity and Magnetism, although every one is aware that the principal facts, both with respect to the one and the other, have long since been made public by their respective authors, and have been abundantly commented upon by others. And nowhere, perhaps, is it more desirable to instil those sentiments to which I have alluded, than within the precincts of those provincial cities which the Association now proposes to visit. The inhabitants of those great emporiums of Commerce and Manufactures are indeed often enough reminded that processes directed by the guidance of Chemistry and Mechanics constitute the very basis of their prosperity, but they are too apt to regard these and other kindred sciences, as the instruments merely of material wealth, and to deem it superfluous to prosecute them further than they are seen to conduce to that one end. That such notions are short-sighted, even with reference to the practical applications of the Arts, it would not be difficult to show; but I am ambitious to place the question on a higher ground, and the presence amongst us of such individuals as I have mentioned, will do more towards that object than volumes of argument would effect. It will convince us at least, that other roads to distinction besides that of mere wealth are opened to us through the instrumentality of the Sciences, for although, thanks to the spirit of the age, which in this respect at least stands advantageously distinguished from those preceding it, the discoverers of important truths are not, as heretofore, allowed to languish in absolute poverty, yet the debt which Society owes to them would be but inadequately paid, were it not for the tribute of respect and admiration which is felt to be their due.

It has indeed been sometimes objected, that too large a share of public attention is in this age directed to the Physical Sciences, and that the study of the human mind, the cultivation of literature, and the progress of the Fine Arts have been arrested in consequence. In what degree the accusation is well founded, this is not the place to inquire, although, when we look round upon the many literary characters that adorn this age, we should rather suppose the remark to have arisen from the increasing interest in Science, than from any diminished taste for other studies. If this complaint however had any foundation in truth, it would only supply a stronger argument in favour of an Association like the present, the express object of which is to correct that narrowness of mind which is the consequence of limiting ourselves to the details of a single science, or, it may be, to a single nook and corner of one, and therefore to render the prevailing taste of the times more subservient to mental culture, and therefore a better substitute for the studies it is

alleged to have superseded. An Association too, which, with no narrow and exclusive feeling towards those pursuits which it is designed to foster, extends the right hand of fellowship to men of eminence in every department upon which the human mind can be exercised, and which would have felt that no higher honour could have been bestowed upon its present Meeting, than by the attendance of the great poet, and the great sculptor, who own Bristol as their native city.

To alter indeed the character of the period in which we live, is as much beyond the efforts of individuals, as to fix the time of their birth, or the country and station in which their lot is cast; and it is perhaps inevitable, that an age and country so distinguished above all others for the advancement of arts and manufactures, should attach an increased importance to those sciences on which both the latter are dependent. But it is at least consolatory to reflect, that Providence has attached to every one of those conditions of society through which nations are destined to pass, capabilities of moral and intellectual improvement, and that the very sciences which so amply minister to our physical enjoyments, also afford the means of those higher gratifications which spring from the exercise of the taste and imagination. Thus, although it may not be easy for the citizen to indulge to any extent in studies alien from the pursuits which engross his hours of business, yet it cannot be deemed incompatible with the latter, to mount up to the principles of those sciences which are connected with the arts he practises; to study their relation one to the other; and to acquaint himself with the steps by which they have reached their present eminence. It cannot but be useful to the chemical manufacturer to study the laws of that molecular attraction which binds together the elements of the substances which he prepares; to the mechanic to examine the process of the arts in connexion with the general laws of matter; to the miner or land-surveyor, to inform himself with respect to the physical structure of the globe; to the agriculturist, to become acquainted with the principles of vegetable physiology, and the natural relations of plants.

For my own part, intimately connected as I am, both with the first of the commercial cities, and also with the first of the universities, that welcomed the British Association within its precincts, warmly interested in the prosperity of both, and officiating as Local Secretary on either occasion, I have felt personally gratified at seeing the selection of these places justified by the cordiality of our reception in both, and at witnessing the new vigour which has been infused into the Association, in consequence of the support it has therein received. But how much will that gratification be augmented, if it should be found hereafter, that the benefit in either case has been mutual; that these Meetings have cemented those bonds of union between the academical and the commercial portion of the British community, which it is so desirable to maintain; and that, whilst the University to which I belong has reaped advantage, by having its attention called to the interest felt in physical sciences generally throughout the kingdom, my fellow-citizens here will in like manner catch the spirit which pervades our body, and will engage in the pursuit of science, with a juster conception of its high objects, and with a zeal and devotion to its cause, which will not be less practically useful, because it is stimulated by a more disinterested love of truth; less capable of ministering to the operation of the arts, because it is also rendered subservient to mental discipline and improvement.

John Taylor, Esq. Treasurer of the Association, then read the account of the receipts and expenditure, made up to the middle of July this year; from which it appeared that the

Balance in hand at the last Meeting was	£509	16	3
Received from Members at Dublin, and since	2173	0	0
Amount of Interest, Dividends, &c.	127	10	0
Sale of Volumes of their Transactions	375	8	9
	£3185	15	0

The Expenditure was as follows :—

Expenses of Meeting at Dublin	£235	18	0
Various local expenses	121	19	0
Purchase of £1000. Three per Cent. Consols	916	5	0
Salaries	230	0	0
Grants for various Scientific purposes	457	0	0
Printing Third Volume of Transactions	517	16	0
Other Printing	54	18	0
Sundries	43	8	7
	£2577	4	7

Leaving a balance in hand at present of about £608.

The total amount of property belonging to the Association at present, including the value of a number of copies of their Transactions, is about 4,564*l*.

Mr. Taylor further stated that the number of tickets issued up to that time was at least 1000, the largest number that had ever been issued at that stage of their proceedings; the probability, therefore, seemed to be, that the number of members attending the Association would be larger than ever they were before, and the pecuniary benefit proportionably greater.

TUESDAY, AUG. 23.

SECTION A.—MATHEMATICS AND PHYSICAL SCIENCE.

The President upon taking the chair this morning called upon Mr. Russell, of Edinburgh, for his 'Notice of a Series of Experimental Researches regarding the Laws of the Motions of Waves excited in Water.'

This notice regarded one department of a series of investigations in Hydrodynamics, in which the author has been engaged for three years.—(See Sec. G, Monday.) It was discovered in the course of these investigations, that the phenomena of waves interfered with the phenomena of resistance, to such an extent, as to render an investigation of the laws of the propagation of waves essential to the farther prosecution of the inquiry into the laws of resistance. The importance also of these inquiries, in connexion with the investigations of Mr. Whewell and of Mr. Lubbock, regarding the tide-wave, gave an importance to the inquiries which had induced Mr. Russell to prosecute the investigation in such a manner, as to render it subservient to the improvement of a department of science, of which the applications are so highly important.

Much confusion has arisen from confounding different species of waves following different laws. Mr. Russell has observed four species;—1. Waves of the first species are seen in what is commonly called ripple on the surface of a pool; these may be called dentated, and are not propagated beyond the place of their generation; 2. Waves of the second species, or *oscillatory* waves, are found when a stone is dropped into a quiescent fluid, and these succeed each other in concentric rings—these are the waves of Newton and Young, and correspond to the second species of Poisson; they are propagated with a velocity proportioned to the magnitude of the displaced fluid; 3. The third species of waves are called breakers, surges, and tidal-bores; and 4. The fourth species of waves, is the solitary wave, analogous to the great tidal wave of the ocean; it is propagated with nearly a uniform velocity. The two last species, the surge and the solitary wave, are the subjects of this investigation. It was observed, 1st, When a considerable and permanent addition is made to the volume of a limited portion of fluid contained in an open reservoir, such addition produces an elevation of the surface of the fluid, which is propagated in the form of a solitary wave, moving with a velocity nearly uniform. 2nd, The velocity of the propagation of such waves, is equal to that which would be acquired by a heavy body, in falling through a space equal to half the depth of the fluid. 3rd, The length of such a wave is nearly constant for a given depth. 4th. The height of the wave varies with its volume, and must be added to the depth of the fluid, in calculating the velocity according to art. 5th, When the height of a wave exceeds twice the depth, its form ceases to be a form of equilibrium, and it breaks. 6th, When the anterior part of a wave is found at a depth less than that of the posterior portion, and the height is greater than twice the depth, the wave curls forward, forming the common surge. 7th, When the width of a channel diminishes in an arithmetical ratio, the height of the wave increases in a geometrical one, until it exceeds twice the depth, when it breaks.

The Members expressed their satisfaction at the ingenious devices by which Mr. Russell had contrived to effect his observations—as where he noted the arrival of the wave at a given position, by placing a white rod across the top of the canal along which the wave was propagated, and watching its image in the approaching wave: while the inclined part of the wave was passing under it, the reflected image of the white rod was transferred, of course, to a considerable angular distance; but as soon as the top came under the rod, a very small portion of it being horizontal, the reflected image assumed a position exactly beneath the rod. By this most ingenious contrivance, he was enabled to determine both the velocity of the wave, and, to some extent, its length, and ultimately its form. The clear manner in which his experimental results explained the change of form of the greater wave, by the incompatible velocities of two waves of a lower height, at first generated, until ultimately the two lesser were absorbed by the greater, and disappeared altogether, was unexpected, and surprisingly exact. The manner in which the wave changed its form, piqueing more and more up, as its height became greater and greater, in proportion to the depth of the canal, until at length, upon a shelving bottom, the form ended in the surge or breaker—the manner in which this explained every minute familiar phenomenon, called forth repeated applause; and we would particularize the curved form of the surge along the coast, determined not so much by the shape of the coast, as by the course marking the equal depth of the bottom below the surface, as soon as that depth reached the surging limit; also the circumstance that the surge frequently made its appearance first at one particular point, and then ran off in a kind of *feu de joie*, sometimes in only one direction along the curved line of surging depth, sometimes in both directions; these, and many other particulars, which we find it impossible to insist on, met with the most minute explanation.

The Rev. Mr. Scoresby, better known to our readers as Captain Scoresby, begged leave to ask Mr. Russell, whether in waves generated in the deep ocean, as in the Atlantic, this explanation would not lead us to infer a velocity of the wave greater than that of a cannon ball. He also begged to suggest to him the consideration of the effect of deep indentations upon the coast, on the waves; he had known, and he described some instances, where the indented form of the coast had a tranquillizing effect upon the waves of the sea outside, so as frequently to produce almost still water.—Mr. Lubbock put several questions to Mr. Russell, for the purpose of eliciting information and explanation upon topics, on which, from the rapidity with which the interesting results succeeded each other, he had not received full satisfaction. He felt peculiar interest in this question, from its direct bearing upon the tides, a subject to which he had latterly much devoted himself; he asked Mr. Russell, whether any of his experiments had been conducted in covered canals, or whether he had observed any facts connected with the effect of the wind, either upon the height or upon the time of propagation of the wave. —Mr. Russell stated, as we understood him, that he had not used covered canals; that he had noted several of the effects of wind upon both the height and velocity of the wave, but he had only time to note these very roughly, but these effects were both considerable and important, and he was even led to expect, that by this influence, will at length be explained among other interesting effects, the surging of the waves in deep seas, which takes place in consequence of the form of the wave being changed into one piqueing up at the top. Mr. Roberts inquired what means of measuring time Mr. Russell employed, and to what part of a second he could observe?—Mr. Russell replied, that he chiefly used ship chronometers, and that he never tried to come nearer than half a second; in that time, as he had in the course of the paper observed, the ordinary wave of the experimental canal he used, progressed about six feet.

Mr. Whewell observed, that experiments had, in some instances, borne the most ample testimony to the correctness of the theory of this intricate portion of hydrodynamics; although, in the greater number of instances, by outstripping that theory, it exhibited the deplorable state of deficiency in which it still existed. The learned Professor congratulated the Section upon the prospect which now brightened

before them, for he had little doubt but that these experimental results, in the hands of some expert analyst, would at length conduct to an advance of the theory which in many branches of science would be of much importance. To himself, the obvious and very close connexion of these researches with the subject of the tides was a matter of the most intense interest. The learned gentleman then proceeded to particularize some leading and valuable examples of that connexion. The propagation of the great tide wave was a direct case of the propagation of the waves of that species, to the tracing of whose laws Mr. Russell had chiefly applied himself. And this led him directly to an answer to one of Mr. Scoresby's questions; for, since that tidal wave could be shown to be propagated from the Cape of Good Hope to the ports along the coasts of Spain in about twelve hours, surely here was an instance of propagation of these waves in deep waters, not only equalling, but far surpassing, the velocity of a cannon ball.

Sir William Hamilton congratulated Mr. Russell upon the most successful issue of his researches thus far, and strongly urged upon him the continuation of them, and even the extension of them. The fund upon which Mr. Russell drew for bearing the expense of his experiments, conducted, as they had obviously been, upon the most splendid scale, of course left nothing to be desired if it still continued unexhausted; but to him it appeared that if there was the least chance of a difficulty in procuring funds, those of the British Association could not be applied to a more legitimate or a more important purpose; and he felt little doubt that, if there was any necessity for such application, it would be successful.

Professor Powell then read a paper respecting the Refractive Indices of several substances.

The Professor commenced by giving an explanation of the term Refractive Index, and pointed out the changes of meaning which it had undergone since the time of Newton. He then explained what was meant by the "dispersive power" of a substance. The determination of the refractive indices for definite rays of the coloured image of the sun, or solar spectrum, marked by the dark lines of Wollaston, from direct observations of their deviations, produced by prisms of various substances, was first proposed by Frauenhofer; and, by the aid of instruments of extraordinary delicacy and exactness, executed by him, obtained for ten media solid, and was carried on by Mr. Rudberg for ten more. The absolute necessity of an extended series of such determinations, was pointed out by Sir J. Herschel and Sir D. Brewster, and was further urged by a special recommendation of the British Association; but Mr. Powell, finding that no other person was stepping forward to undertake the task, stated, that he was himself most reluctantly induced to endeavour to make some progress in this matter, so highly important in a practical point of view. The apparatus of Frauenhofer, besides its extreme complexity, required in the using a skill and accuracy, possessed by few but himself. This, and other considerations, induced the learned Professor himself to procure the aid of Mr. Simms in constructing a much simpler apparatus, the essential parts of which are a graduated circle, with the prism of its centre diameter 10 inches—an achromate telescope, with cross wires at its focus—an arm, projecting from the centre, carries the prism with two motions, one to adjust it to parallelism, with the slit through which the light is admitted, and which is about the 1-20th of an inch wide, and then round its axis. For liquid media, hollow prisms, or troughs, of different angles, were provided, whose inclined sides are of plate glass, formed with truly parallel surfaces, and the angles accurately determined previously. For seeing some of the lines of the spectrum, the light of the sun is required, in which case one or more thickness of purple glass is used, and, by means of the lines themselves, the most perfect parallelism of the prism with the slit can be obtained; and, since the lines become indistinct, except in or near the position of minimum deviation, that position is always adopted; this last deviation is accurately observed by the focal wires. The absolute deviation is observed directly from the zero point of the circle, or that which corresponds with the telescope directed to the slit. This instrument, the Professor stated, although comparatively so simple, admitted of a degree of accuracy, even greater than what he cared to wait for obtaining, his object being a great number of approximations to the truth, rather than the attainment of the utmost precision. He then proceeded to notice the care required in attending to diversities of temperature, since he had found the greatest differences to result from this source. He alluded briefly to the precautions used, and then stated the means by which he had obtained the oils, alcohol, and chemical substances, which were the subjects of these experiments, Mr. Allen and Professor Daubeny giving him the most essential aid in this important part of his labours. The fluids that he examined were oil of cassia, oil of aniseed, sulphuret of carbon, balsam of Peru, kreosote, oil of sassafras, oil of pimento, oil of angelica, sulphuric acid, muriatic acid, nitric acid, alcohol 39=.815, solution of chromate of lead in nitric acid, solution of chromate of potass, of muriate of lime, muriate of ammonia, nitrate of potass, sulphate of magnesia, nitrate of mercury, muriate of barytes, sulphate of soda, muriate of zinc, nitrate of bismuth, nitrate of lead, superacetate of lead, subacetate of lead, distilled water the same as that in which the solutions were made. Of these the refractive indices of many were very high, and most of them of such a nature as to render them scarcely possible to be reduced to the proper form. In conclusion he stated, that many crystals of various kinds of substance, if got pure, would be invaluable for aiding in these researches—he particularized lead. The learned gentleman concluded by some notice of the researches of Dovey.

Sir David Brewster stated many modes in which some of the most perplexing difficulties met with by Mr. Powell might be avoided. He stated, that he could render very imperfect crystals available by covering up their imperfections with China ink; also, we understood him to say, that sometimes he interposed silk; but he universally succeeded in making the dark lines visible, by using the triple oxalate of chromium and lead as an interposing substance. He also remarked, that it would be very important to note accurately, the angle which the ray or beam of light made with the edge of the refracting angle of the prism, and illustrated its importance.—Mr. Powell showed, that in the very construction of his apparatus this was provided for.—Professor Forbes bore testimony to the extreme simplicity of the apparatus, and its capability of accurate investigations. He had only just come from Oxford, where he had had the pleasure of seeing and examining it, and from his own experience he could say, that any one, at all accustomed to such experimenting, might be readily trained to use it.

A paper was then read, contributed by Sir. D. Brewster, 'On the Polarizing Structure of the Crystalline Lens of the Eyes of Animals after Death.'

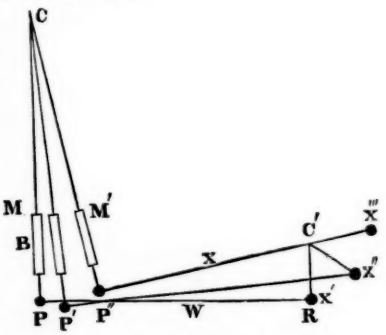
The eyes examined were those of oxen and sheep, the tendency of the lens to indurate was prevented by immersion in distilled water; the polarizing structure was then examined; great changes were stated to take place in that structure in the eyes of aged animals. From these investigations, the writer was led to the results that there is in the crystalline lens a capability of being developed by the absorption of the aqueous humour; that a perfect structure is not produced until the animal frame is completely formed; and that when it begins to decay, the lens changes both its density and focal length, and sometimes degenerates into the disease termed hard and soft cataract. Sir D. Brewster is led to entertain a hope, that these researches may furnish a means of preventing or curing that alarming disease.

The Rev. J. W. M'Gauley then read 'A series of Experiments in Electro-Magnetism, with reference to its application as a Moving Power.'

Previously to the detail of the experiments on this subject, he thought it might be interesting to the Section to relate what he had done since the last Meeting of the Association, in the application of electro-magnetism to machinery. He had intended, originally, to have exhibited the improvements, but should content himself, for the present, with the *detail*, rather than the *exhibition*. He was obliged to confess, that he was the less anxious prematurely to publish results, since he found, that the working model of last year, given to the Section, undoubtedly with the intention of its future improvement, or the pursuance of experiments by other Members, had led on several occasions, to the production of papers, and the exhibition of models, by those from whom it might not be expected—with a pretension to originality, but with no change in the principle, and almost none in the details.

The working model exhibited to the Sections at the last Meeting of the Association must be acknowledged as a proof, to some extent, at least, of the applicability and the manageableness of electro-magnetism as a moving power; but the question then remaining was, whether or not it was likely to be applied to useful purposes; for this, several things remained undone.

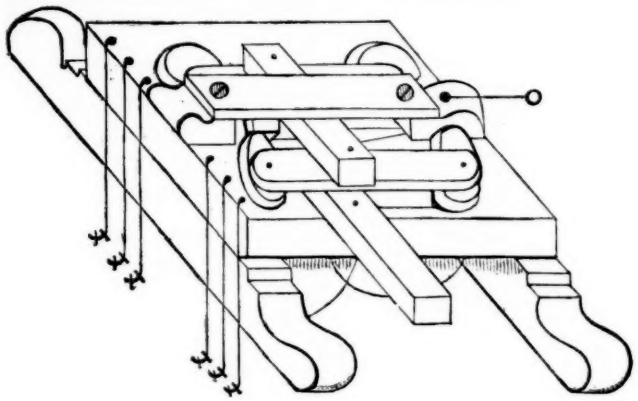
Powerful magnets were to be constructed. The ordinary formation of electro-magnets furnishes us, at best, with an apparatus clumsy in the extreme, and, as we shall see, of very limited power. This arises from the very nature of an electro-magnet; for the lifting power may be very great, although the attracting power at a small distance may be very trifling. There must be a limit, also, to the size of these magnets, for, if the mass of iron be too great for the helix, it is not saturated with magnetism, and the helix cannot be unlimited, as, beyond a certain distance from the iron, its action is nothing,—in some cases, perhaps, as we shall see, even injurious. The effective distance of the helix from the iron cannot be great, since its action, probably, decreases in the inverse proportion of the square of that distance. This difficulty cannot be obviated, as some have imagined, by causing the electrical currents to circulate through the mass of iron, uniting together a number of coiled bars. This would present an arrangement probably similar to a permanent magnet, the masses of iron acting on each other by induction, the reversion of the poles would be very slow, or altogether impossible. The *action* of the magnets, rather than their *masses*, must be united; but in this new difficulties occur. Their action must be simultaneous, or the machinery will be broken, or ineffective; the time *after* reversion, and during which a *bar can* be thrown off a magnet, is extremely short—hence one reason why it is difficult to unite the action of several magnets. But let us suppose that we have obtained a simultaneous reversion of the poles and throwing off of the bars—a thing totally impossible, he conceived, from the number and complication of circumstances by which it is influenced—how shall this action be applied to machinery? If the fly-wheel of a steam-engine, from the shutting off of the steam, be not impelled by the engine while it continues in motion, it drags the piston, uninjured, through the cylinder; but suppose something to retain the piston in one position, without stopping the wheel, the effect were highly injurious—this is exactly what must frequently happen in electro-magnetism. It is impossible to reverse the poles even of one magnet, in such a manner that the position of the bars shall always correspond with the position of the crank and fly-wheel.



Let M M′ be two magnets, M M′ be the space through which B, the bar, travels in causing half the revolutions of the crank C′ x, while B is moving, so that its extremity shall be at P′; then C′ x shall have become C′ x″ while it is going to P″, C′ x′ shall become C′ x‴, but if when the crank C′ x is in the position C′ x‴ one of the dead points, the bar is not ready to leave M′; or, in other words, if the magnet

which holds it be not ready at once to send it off—a thing very probable—the fly-wheel continues to revolve by its own inertia, and the machinery is broken, or the bar is torn from the magnet, which often has a curious and perplexing effect on the reversion of the poles.

A better reversing apparatus was to be obtained. The one of last year, though perfectly successful, required the agency of mercury, which, for many reasons, is objectionable; it becomes oxidated, then contact is imperfect, and the level in the cups, which is of the last importance, is destroyed: it is liable to a thousand accidents, not to speak of its destroying the wires of the apparatus itself.



Again, the form of the apparatus, whether mercury be used or not, must be *changed*, and the *principle* of the one now exhibited to the Section adopted, since the apparatus, which will reverse the poles of one magnet, will not with speed or certainty reverse the poles of two or more, when worked by the engine itself. The apparatus shown to the Section had been used with great success in the reversion of the poles of four powerful magnets.

The attachment of the reversing apparatus to the machine becomes difficult, when more than one magnet is used, for reasons with which he would not then occupy the Section. He believed he might mention, that he possessed an engine of considerable power, in which these difficulties were overcome.

The experiments he should detail to the Section were numerous and complicated; he had taken every means to secure their accuracy; some of them appear anomalous, but were undoubtedly modified by circumstances, many of which are so obscure, that he has not been able yet to detect them. He remarked, that it was obviously important to make experiments in considerable number, and on a large scale, since the former secures a greater accuracy, the latter the notice of results which, from their minuteness, might otherwise escape observation. His inquiries resolved themselves into two points—the nature of magnetism—the best means of producing it. The means of overcoming the difficulty arising from the necessarily limited size of the iron and the helix, he might probably treat at a future period.

To place everything in exactly the same circumstances, without which a fair comparison could not be made, the battery was cleaned, the charge renewed, and the arrangements examined before each of the following experiments, and the materials of the charge were, as far as possible, not merely similar, but identically the same.

FIRST SERIES. THE HELIX.

No. 1.—A horse-shoe soft iron bar, 13¼ inches across, to the extreme edges of the poles, 5¼ interior, 7⅞ exterior length. The diameter of the bar 2½ inches, its weight 29½ lb. The keeper used was 13¾ inches long, 2½ wide, ⅜ thick; its weight, with ring, 7¾ lb. This keeper was used in all experiments with magnets of the same size. This, like all the other magnets, was well coated with sealing-wax varnish; it was coiled with 1690 feet of No. 13 copper wire, in 10 equal coils. The battery was one foot square, double cell, charged with 1 in 50 sulphuric acid, 1 in 100 nitric acid, and water.

Keeper distant from poles 3/10 inch, it lifted 4¼ lb.
3/20 ..... 9¼

Magnet No. 1, of 5th Series, connected with the battery, at the same time, lifted in contact ... 9¾ lb.

He removed one of the coils, used the same battery, and a similar charge, at the distance,
3/10 inch, it lifted 7½ lb.
3/20 37
¼ 26½

The spark, on breaking contact, was very brilliant.

Same magnet, battery, and similar charge, but the poles of the battery and the magnet thicker wire, it lifted at ..... 3/10 ..... 14½ lb.
3/20 24
¼ 32½

Same magnet, battery, and charge, but thicker zinc plates, it lifted at ..... ¼ ..... 32½ lb.

Same magnet, &c. thinner pl. ¼ 60

Same magnet, battery, and charge, one coil of the same wire, 150 feet in length, ¼ perceptible in contact ..... 12½ lb.

Same magnet, battery, charge, &c., 2 separate coils, each 150 feet ..... 5⅛ lb.

Same magnet, battery, charge, and coils, the two coils formed into one of 300 feet, at same distance... 2 lb.

Same magnet, battery, and charge, 3 coils, making together 450 feet..... ⅛ ..... 2½ lb.
¼ 3½
½ 4½
1 ..... 9½

The wire was coiled in larger quantity near the poles.

Same magnet, battery, and charge, 4 coils, 600 feet, it lifted at ..... ⅛ ..... 2½ lb.
¼ 3½
½ 3½
1 ..... 12½

Wire coiled equally over the magnet.

Same magnet, battery, and charge, 5 coils, 750 feet, at ..... ⅛ .. perceptible.
¼ ..... 3½ lb.
½ 2½
½ 4½
1 ..... 12½

Wire rather more on one pole.

Same magnet, battery, and charge, 6 coils, 900 feet, at ..... ⅛ ..... 3½ lb.
¼ 3
½ 3
½ 7½
1 ..... 18½

Same magnet, battery, and charge, 7 coils, 1050 feet, at ..... ⅛ } perceptible.
¼ }
½ ..... 2½ lb.
½ 5½
1 ..... 19½

Wire coiled evenly. Battery diminished in energy almost immediately.

Same magnet, battery, and charge, 8 coils, 1200 feet, at..... ⅛ ..... 7½ lb.
¼ 9½
½ 4½
½ 8½
1 ..... 30½

Wire coiled more on the poles. When it lifted at 1 30½ lb. its energy was so far diminished as to be at ½ inch scarcely perceptible.

Same magnet, battery, and charge, 9 coils, 1350 feet, at ..... ⅛ ..... 5½ lb.
¼ 6½
½ 9½
½ 8½
1 ..... 21½

Same battery, magnet, and helices, charge 300 drachms water, 10½ sulphuric, 5½ nitric acid.
1 ..... 17½ lb.

The wire was coiled evenly, and filled the inside curve of the magnet.

SECOND SERIES.

Same magnet and battery, charge 400 drachms water, 8 sulphuric, 4 nitric acid; all the coils remaining on those nearest to the iron connected with the battery, lifted, at 1 inch, the following quantities.

Average of a great number of Experiments:—

9 coils..	1350 feet..	19¾ lbs.
8.....	1200.....	26½
7.....	1050.....	22¾
6.....	900.....	18¾
5.....	750.....	13¼
4.....	600.....	11¼
3.....	450.....	7½
2.....	300.....	3¾
1.....	150.....	2¼

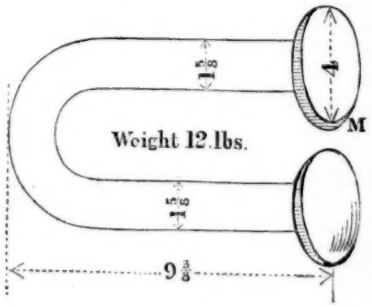
The ninth helix on this occasion occupied nearly the same place as the tenth on the former, and, like it, was injurious to the effect.

THIRD SERIES.

Same magnet, battery, charge, and helices,
2 helices arranged so as to form but one, lifted at 1 inch ..... 5¾ lb.
3 helices forming one 4½
4..... 7½
5..... 6½
6..... 0
1..... 4¾

FOURTH SERIES.

Magnet No. 1, horse-shoe bar, 9⅜ exterior length, from extremity of poles to highest joint of exterior



curve, diameter of bar 1¼ inches, discs surmounting the poles 4 inches diameter, ½ thick. Coiled with 500 feet of No. 15 iron wire, in 5 equal helices. Keeper 9½ inches long, 4 1 wide, ⅜ thick, weight, with ring, 9¾ lb., battery 1 foot square, double cell, charge 400 drachms water, 8 sulphuric, 4 nitric acid. Flannel casings of the zinc plates new.

At 1 of an inch this magnet lifted 6 lbs.

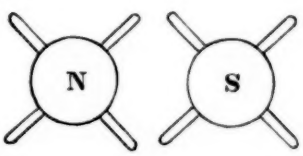
Magnet No. 2, same size, &c. coiled with 500 feet of No. 13 copper wire, in 5 equal helices, same arrangement, battery, and charge.

This magnet lifted at ¼ inch.....4 lb.
1 .....7½

Magnet No. 3, same size, and coiled with 500 feet of No. 12 iron wire, same arrangement, battery, charge, &c.

This magnet lifted at ¼ .....1½ lb.
1 4

When the keeper was in contact with this magnet, reversion of the poles was not effected. The reversing apparatus used was that exhibited to the Section last year.



Magnet No. 4, same size, &c. except that the ground discs at the poles were replaced by pins, for the purpose of retaining the wire. Coiled with 500 feet No.12 iron wire, in 5 equal helices, same battery, charge, &c., hardly any magnetic effect, although Magnet No. 2 of this series, when connected at the *same* time with the battery, was powerfully excited.

Magnet No. 1, same battery, &c. Flannel cases of zinc plates had now been used several times,

at $\frac{1}{16}$ inch .....lifted..2½ lb.

Magnet No. 2 again at $\frac{1}{8}$ inch 5½

$\frac{1}{16}$ 14½

I had frequently remarked an electro-magnet to improve by use.

Fifth Series.

Magnet No. 1, soft iron square horse-shoe bar, interior length 5$\frac{1}{16}$, exterior 7$\frac{3}{4}$ inches, $\frac{1}{4}$ inch square, coiled with one helix of 90 feet No. 13 copper wire.

Magnet No. 2, *cast iron*, same size, similar helix.

Magnet No. 3, wrought iron, same length, but round bar, $\frac{1}{4}$ diameter; coiled with a similar helix in two lengths, each coiled over the entire bar.

Magnet No. 4, same size, &c., coiled with 60 feet of same wire in two helices, one helix on each half of the bar, the coils increasing in number from the centre to the poles; battery 1 foot square, double cell, charge 300 drachms water, 6 sulphuric, 3 muriatic acid. This charge remained unchanged throughout this series. Keeper No. 1, 5$\frac{3}{4}$ inches long, $\frac{3}{8}$ wide, $\frac{1}{2}$ thick, weight 12$\frac{1}{2}$ oz. Keeper No. 2, same length, double thickness, weight 18$\frac{1}{2}$ oz.; there was, of course, but one ring to the larger as to the smaller keeper, which accounts for the former not being double the weight of the latter.

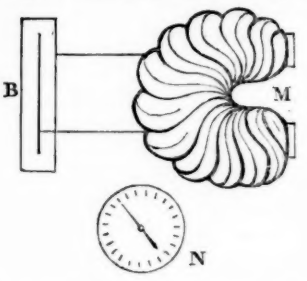
	lb.	oz.
Magnet No. 1, with keeper No. 1, lifted	25	3½
2	8	11½
3	39	3½
4	39	3½
3	25	3½
2	7	3
1	30	2½

Magnet No. 1, with keeper No. 2, lifted 32.11; an accidental stirring of the battery accounts for the increase.

Magnet No. 4, with keeper No. 1, lifted	34	3½
2	26	3
1	24	2½
2	15	3½

Sixth Series.

To test, with as much accuracy as possible, the comparative excellence of various charges, he tried the following experiments. The galvanometer consisted of a small, but exceedingly good, electro-magnet M, a



battery, B, two inches square, single cell, zinc plate ¼ inch thick at commencement of experiments, and a delicate needle, N, made to stand at zero when the battery was not connected with the magnet. The least magnetism produced by connecting the battery with the magnet, tended to deflect the needle. He preferred this galvanometer, on this occasion, to the ordinary one, for many reasons. The battery was well cleaned after each charge. The first deflection on connecting the battery with the magnet, carefully noted the number of degrees at which the needle rested, and the decrease of deflection every quarter of an hour for six quarters. Rain-water was used. The specific gravity of the sulphuric acid 1840; nitric acid 1410; muriatic 1175.

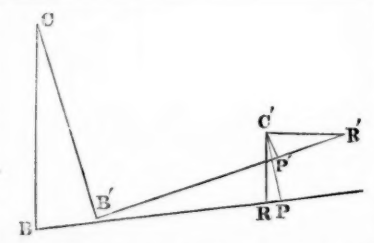
The solution of caustic potash contained 2 in 7½ of the liquor.

Table of Results of the Experiments.

	First Deflection.	Needle settled down to	Deflection in 1st quarter decreased to	In 2nd quarter.	3rd.	4th.	5th.	6th.
Rain water and nitric acid .. 1 in 21 parts	90°	52°	32½°	16½°	11½°	4½°	3½°	3°
1 in 31	38	27¾	15½	13	6½	5½	3½	2½
1 in 41	46	46	31	14½	8½	3½	2½	¼
1 in 51	38	34 *	26½	12¾	7½	5	3	½
Rain water and sulphuric acid 1 in 21	88	44	15½	13½	9½	8½	8½	7
1 in 31	49	30½	10	7½	6½	5	4	3½
1 in 41	67	44	15	8½	5½	4½	4	2½
1 in 51	33½	24	3½	2½	2	0	0	0
Rain water and muriatic acid 1 in 41	97	33½	7½	6½	3	0	0	0
1 in 51	58	22½	9	6½	0	0	0	0
Solutions of caustic and potash	80	19	7¾	7	5½	4½	2½	1¾
Rain water and sulph. acid 1 in 41½ nitric 1 in 83	98	48	24	11	7½	6	5½	4¾
1 in 26½ .... 1 in 53	82	40	24	15½	9½	7½	6½	5½
1 in 161½ .... 1 in 323	23½	20½	7½	4	1¾	½	0	0
1 in 201½ .... 1 in 403	50	23	2½	0	0	0	0	0
Rain water and muriat... 1 in 52 sulph. 1 in 52	35	25	7½	6½	5½	4½	4½	3¾
nitric 1 in 52	115	62½	28½	18½	15½	10	0	0
1 in 52½ sulph. 1 in 103	85	46½	10	6	3½	0	0	0
Solution of caustic potash, and sulph. ... 1 in 51	88	45	8½	6¾	4	0	0	0
and nitric ... 1 in 51	43	0 †	0	0	0	0	0	0
and muriat... 1 in 51	25	18	0	0	0	0	0	0
Sea water from Dublin Bay	½	0	0	0	0	0	0	0
Sea water and sulphuric acid ..... 1 in 51	35	26	13	8	5¾	3½	0	0
and nitric acid..... 1 in 51	95	55	33½	15½	9½	0	0	0
Rain water and nitric, 1 in 53, sulph. 1 in 53, and muriat. 1 in 53	95	59	40	16	10	3	0	0

* In two minutes it ran up to 85°, settled down to 51°.
† Would not deflect the needle even with renewed charge.

Mr. M'Gauley thought it would be unbecoming in him to suggest anything to the British Association; but he believed nothing would be more conducive to the interests of science, than that the Association should cause to be instituted a series of experiments on the Galvanic Battery and its Charge, which would set all questions on the matter at rest for ever. Before he left this part of the subject, he thought it well to recall the attention of the Section to the nature of the power obtained by electro-magnetism. In steam, one great cause of the varying power of the engine arises from the varying leverage of the crank. Let B and B′ be positions of the extremity of the piston-rod, C′ B and C′ B′ corresponding positions of the crank, the leverage of the crank is measured by the perpendicular C′ P and C′ P′. It varies as that perpendicular. But in electro-magnetism, the force at B, say the bar traversing between the magnets, is always varying. He would not then enter into some curious results obtained by calculation on this matter.



He had been anxious to satisfy himself, by his own experiment, of the truth of the law of magnetic attraction being in the proportion of the inverse square of the distance, but abandoned the inquiry for the present, when he found that a magnet, with a seemingly appropriate bar, would lift at one-sixteenth of an inch only five pounds; though with a different bar it lifted the same weight at twelve times the distance; and that the greater the distance through which powerful attraction might be exerted, the less the lifting power appeared.

Identity of Magnetism and Electricity.—In examining the identity of electricities derived from different sources, it seemed to Mr. M'Gauley that we sometimes forget that electricity may be modified both as to quantity and intensity; and that if either be changed, or both, we cannot expect the same results. To test, therefore, the identity of any agent with electricity, we must not use those means which are the measure of, or dependent on, either quantity or intensity; for if in such experiments the electrometer or galvanometer be not affected, we only arrive at a negative conclusion—that if the agent under consideration be electricity, it differs from the ordinary electricity in quantity, intensity, or both. For though we never had been able with galvanism to cause the leaves of the electrometer to diverge, or with machine electricity to deflect the galvanometer, or with electricity to produce magnetism, or with magnetism, electricity, with electricity to produce heat, with heat, electricity,—their non-identity would by no means follow. To examine with ease and certainty the identity of anything with electricity, we must find some property of electricity, which is not modified by, nor dependent on, quantity or intensity. We know, and chemistry furnishes us with one proof, that the elements of things may be the same as to quantity, and as to the intensity of mutual action; and yet may be productive of vastly different effects. Thus we know, that from two equal volumes of carbon and hydrogen, may be formed at least three very different substances.

The following facts seem to afford additional evidence of the perfect identity of electricity and magnetism; and that magnetism does not require, nor suppose, the circulation of electrical currents.

1st. A shock and spark are obtained by means of an electro-magnet only after battery communication is broken; for no matter how long this communication is maintained, neither shock nor spark shall be perceived. 2ndly. The shock and spark are not the effects of the battery; for to obtain a shock—(this shock he had not seen remarked by any experimentalist)—it is not necessary to form a part of the communication between the *copper* and *zinc*, but merely between the extremities of the helix, or between either extremity of the helix and the copper *or* zinc of the battery. 3rdly. The shock and spark do not arise from the magnetism of the bar included in the helix, since the more perfectly the bar is de-magnetized in breaking contact the better. Besides, it is curious that a powerful shock and brilliant spark may be obtained without any iron, and from a heap of wire thrown without any heliacal arrangement. This, Mr. M'Gauley remarked, would lead to a very simple and effective electrical apparatus, one easily managed, and always ready for use; the length and number of the coils, with a given calorimeter, has an effect on the shock and spark. Mr. M'Gauley exhibited to the Section wire coiled with the greatest accuracy, by a machine he had constructed, which was capable of covering any wire, manufacturing pianoforte strings, &c., in any length, without any care on the part of the operator, to the enormous extent, if necessary, of 7000 feet per hour. The wire which he exhibited, as several in the Section knew, was not more perfectly manufactured than the many thousand feet he had covered lately. He thought the shock and spark might arise in this way: a current of electricity passes through the wire from copper to zinc; its inductive action on the wire ceases suddenly, by the contact with the battery being interrupted; the disturbed equilibrium of the wire is suddenly restored. The electricity of the battery seems, in passing through the helix, to acquire an augmented intensity; but from these facts it is evidently not so. 4thly. The spark and shock appear to demonstrate that currents do not circulate around the magnet. If they do, as is evident, they are capable, as we know from secondary currents, of producing a spark and shock. The helix, of itself, is capable of these effects: let the helix and the magnet act conjointly; these effect sought to be doubled; the contrary is the fact; they may be annihilated, and they ought, for the magnet, by its electrical action, retains the helix in a state of ex-

citation. The universal—at least in other cases—law of electrical induction, if applied to magnetic phenomena, easily explains them. He did not think it by any means certain, that electrical action consists in the *transmission of a fluid*, and not the mere arrangement of particles: this idea seemed opposed by an experiment he made some time ago. He never could believe that the action of the galvanic battery consisted in the passage of electricity through the fluid from zinc to copper, and along the connecting wire from copper to zinc; he thought that the repulsion which sent the electricity through the fluid—an imperfect conductor—ought to prevent its return along the wire. He constructed a small box of wood, being a cube internally of three inches, divided it into twelve water-proof cells by well-cemented glass plates; placed in the cells six copper and six zinc plates, one in each, in the usual galvanic order; filled the cells with a charge of 1 in 50 sulphuric acid, 1 in 100 nitric acid, and water, and connected the extreme plates with a delicate galvanometer, but no effect was produced, except when the copper and zinc were in the same cell, or the cells were in conducting communication; but he did not deem this experiment conclusive against his idea, since, although induction might occur from particle to particle, through an imperfectly conducting fluid, it by no means follows this inductive influence should take place through the particles of glass, since the very insulating power of glass, or other substances, may arise from the incapacity of their particles for electrical arrangement.

If it be true, that electrical effect is the arrangement, and not the transmission, of particles, he thought we might easily understand the agitation of the muscles of a frog, caused in *breaking* contact with a galvanic battery, even of a single circle; the dangerous effects to those in the neighbourhood of the discharge of lightning from cloud to cloud; and the spark and shock obtained from a quantity of wire—all of which probably arise from the same cause, and are the consequence of the same universal law.

Professor Ritchie rose to remark that without intending to convey the least censure on the gentleman, he could not but observe, that he had been so entirely occupied with his own researches as not to have attended to anything done by others, for there was really nothing new in this paper—and he gave examples.

Professor Stevelly remarked, that if the only objection to it were the crank and magnetic pendulum not working together, in a large machine that could be at once remedied, by what was well known in practical mechanics, a slipping coupling, as, when the steam-engine and water-wheels were made to work together, was generally done, or as in the winding part of the common clock. The great objection was the small distance through which the power worked, one-sixteenth of an inch; thus, even if a magnet could be produced that would lift 1,000lb, would still render the numerical value of the horse-power almost evanescent compared with the steam-engine.

Section B.—CHEMISTRY AND MINERALOGY.

Mr. Exley's paper on the reduction of Chemistry to mathematical principles, was the first read. Mr. Exley commenced with a division of atoms, into what he denominated the *tenacious*, the *ethereal*, and the *electrical*; the first being distinguished by possessing the greatest, the last by enjoying the least *absolute* force, while in this particular the electrical occupied an intermediate position. In reference to these atoms, two propositions were then laid down, the first of which affirmed the atoms to attract each other according to the inverse square of the distance up to a particular point, when the attraction was converted into repulsion; the second, that dissimilar atoms differ in the relative energies of their attractive and repulsive forces, though these forces vary according to the same law. Mr. Exley having compared his views with those of Newton and Boscovich, and pointed out the particulars in which they agreed, and in which they differed, proceeded to state the grounds which led him to conclude that water was a ternary, not a binary compound; or, in other words, that sixteen, not eight, was the atomic weight of oxygen.

Having disposed of these preliminary topics, Mr. Exley entered upon the developement of his views in the form of a series of sixteen propositions. It is scarcely necessary to say, that this part of his paper does not admit of popular explanation; and it would be presumptuous in any individual to undertake the task, who was not deeply versed in the mathematical sciences, and who had not had the advantage, not only of perusing, but of studying the profound researches of Mr. Exley. We have, however, no hesitation in asserting that these researches are deserving of mature consideration. According to Newton, before a hypothesis is admitted, it must be proved *true*, and adequate to the explanation of phenomena. The former test is sometimes very difficult of application, and would be particularly so in the present instance. But, as respects the latter criterion, it must be admitted to pronounce in favour of Mr. Exley's theoretical postulates; for, by following *these* out, and applying to them mathematical reasoning, he is enabled to anticipate and explain a variety of the most important facts in chemistry and general physics. Thus, he deduces with facility from his principles the ordinary laws of chemical combination, Gay-Lussac's law of volumes, and even the variations of volume, which the gases are known to experience when submitted to various temperatures and pressures. But the most striking evidence of the truth of his theorems, adduced by Mr. Exley, remains to be mentioned. He has calculated by his abstract methods the specific gravities of fifty-seven substances, supposed in the gaseous state, (some, such as alcohol, oil of turpentine and camphor, being compounds of an extremely complex nature,) and found the results to correspond as closely as could in such investigations be expected, with those obtained by direct experimental means. Doctors Dalton and Thomson of Glasgow, as well as other competent judges, bore testimony to the ingenuity and talent shown in Mr. Exley's paper.

Mr. Babbage next exhibited a thermometer, recently discovered in Italy, and supposed to be one of those originally manufactured for the Societa del Cimento. It appeared to be filled with alcohol. The bulb was spherical, and the stem was divided into fifty equal parts by beads attached to it by fusion at equal distances. These instruments, as is well known, being graduated without reference to fixed points, do not give indications comparable with those of the modern thermometer. Libri, it is generally understood, and the circumstance was stated by Professor Babbage, has attempted the interpretation of the scale of these instruments, partly by a comparison with each other of ancient and modern meteorological registers, and partly by taking with them the temperatures of certain tepid waters in the Pyrennees, which had been previously examined by the Florentine Academicians. Dr. Daubeny observed upon the inaccuracy of the latter method, as that springs undoubtedly undergo, in process of time very considerable changes of temperature.

An essay on Gaseous Interference, by Dr. Charles Henry, was next read.—It is universally known to chemists, that if oxygen and hydrogen be mixed, and brought into contact with metallic platinum in the state of wire or foil, or more especially in the spongy form, the combination of these gases is very rapidly achieved, and, if mixed in the proper proportion, they are converted, usually with the phenomena of ignition, altogether into water. It is also well known, and was first noticed by Dr. Turner, that if into an atmosphere of oxygen and hydrogen, mixed in the ratio necessary for forming water, certain other inflammable gases, such as carbonic oxide and olefiant gas be introduced, the combination of the oxygen and hydrogen is, if not altogether suspended, at least materially interrupted. This is what Dr. Henry denominates *gaseous interference*. The cause of this remarkable effect has, at different times, attracted the attention of eminent chemists. Dr. Turner has ascribed it to the soiling of the platinum by the interfering gas, Dr. Faraday to some peculiar condition induced in the metal; while Dr. Henry himself, at a period long prior to the present, conceived it to arise from the fact of carbonic oxide and olefiant gas, having a stronger affinity than hydrogen for oxygen gas. In his present paper, Dr. Henry investigated the entire question. The prominent facts and inferences appeared to be that carbonic oxide retards and limits, but does not altogether prevent the action of platinum on the usual explosive mixture, and the same may be said of olefiant gas. The *interfering* power, however, of the former is vastly greater than that of the latter, their ratio being represented by the numbers 18 and 1. In the case of carbonic oxide, carbonic acid is always produced, the amount depending on the form of the platinum employed, the quantity of the interfering gas, and the temperature at which the experiment is conducted; and, as a general rule, it may be laid down, that the interfering influence of the gas bears an inverse relation to the energy with which the platinum acts, and the degree of heat—conditions, however, which may be considered as identical. The diminution, and even disappearance, of interference at high temperatures, Dr. Henry attributes to a relative augmentation of the affinity of hydrogen for oxygen, an hypothesis indeed established by other and independent facts.

That Dr. Henry's theory of gaseous interference is the true one, he infers from the general fact of no gases exercising any such influence but those which have an affinity for oxygen; and that it is strictly true, at least in the case of carbonic oxide there can be no question, seeing that some of the oxygen is actually employed in the production of carbonic acid.—Dr. Turner expressed his conviction of the value of Dr. Henry's paper, of the ability with which it was drawn up, and of the correctness of the solution of the problem of interference, and such appeared to be the prevailing opinion.—In the course of the paper several other interesting facts, of a collateral description, were stated, and, amongst others, that platinum causes, though slowly, the combination of a mixture of oxygen and carbonic oxide, but that the process is facilitated by the introduction into the jar of a little caustic potash. This latter circumstance he attributed to the removal of the carbonic acid by the potash as fast as it was produced, but Dr. Daubeny, with much probability, viewed it as a case of disposing affinity.

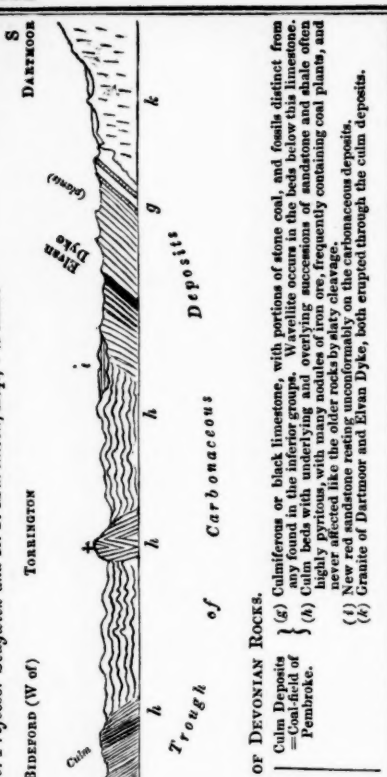
Mr. Herapath then read a paper on Arsenical Poisons, and drew the attention of the Section to the case of Mrs. Burdock, in which he was professionally employed, and which proved to be one of Realgar. While engaged in chemical investigations connected with this case, he ascertained that Realgar is convertible into orpiment by hydrosulphuric acid, and the soluble hydrosulphates, and that it undergoes, as might have been anticipated, an analogous change in animal bodies submitted to putrefaction. Mr. Herepath also stated, if we understood him rightly, that Realgar favours the conversion of animal matters into adipocire, a fact, undoubtedly of a very novel description, and one not very reconcileable with those researches of Chevreul and Gay-Lussac, which have demonstrated this fatty substance to be an ammoniacal soap. This gentleman concluded by exhibiting some experiments illustrative of his methods of toxicological investigation.

Section C.—GEOLOGY AND GEOGRAPHY.

Dr. Buckland in the chair.—The first paper was 'A Classification of the old Slate Rocks of Devonshire, and on the true position of the Culm deposits of the central portion of that county,' by Professor Sedgwick and Mr. Murchison.—The authors began by observing, that this was a mere outline of a more detailed memoir on the physical structure of Devonshire, which they were about to lay before the Geological Society of London. In the published geological maps of that county, the whole system of the older slate rocks was represented under one colour, without any attempt at subdivision; and one colour also represented different limestones, without any discrimination. The object of the authors was, to remedy these defects,—to ascertain and represent the true position of the successive deposits and their natural subdivisions, so as to compare them with corresponding deposits in other places. They also wished to determine the true place of the remarkable carbonaceous deposits of central Devon, which had been previously regarded as belonging to the lowest portion of the grauwacke formation. A section was exhibited of part of that county, from the north coast to one of the granite peaks of Dartmoor immediately south-west of Oakhampton.

[*A Diagram of the Section will be found in the following page.*]

Classification of the older Rocks of Devonshire, by the Rev. Professor Sedgwick and R. I. Murchison, Esq., V.P.R.S.



ASCENDING SERIES OF DEVONIAN ROCKS.

Culm Deposits = Coal-field of Pembroke.	(g)	Culmiferous or black limestone, with portions of stone coal, and fossils distinct from any found in the inferior groups.	
	(h)	Culm beds with underlying and overlying successions of sandstone and shale often highly pyritous, with many nodules of iron ore, frequently containing coal plants, and never affected like the older rocks by slaty cleavage.	
	(i)	New red sandstone resting unconformably on the carbonaceous deposits.	
	(k)	Granite of Dartmoor and Elvan Dyke, both erupted through the culm deposits.	
Cambrian Rocks.	(a)	Slaty schists, with some calcareous courses and organic remains.	
	(b)	Purple, red, and grey sandstones, with beds of iron ore in upper members—peculiar fossils near their junction with the overlying limestones. Veins of lead and copper.	
Upper Cambrian or Devonian Rocks.	(c)	Calcareous group of Combe Martin & Ilfracombe—fossils very abundant—slaty cleavage.	
	(d)	Slates with quartzose veins and beds—incoherent schists, &c. Manganese mines.	
Silurian Rocks.	(e)	Slaty sandstones and schists—cleavage passing through the beds of organic remains.	
	(f)	Ditto, with concretionary limestones, and many well-known Silurian fossils, chiefly of the lower part of the system.	

In the ascending order this section exhibits—

1. A system of slaty rocks, containing a vast abundance of organic remains, generally in the form of casts. These rocks sometimes pass into a fine glossy clay slate, with a true transverse cleavage; sometimes into a hard quartzose flagstone, not unusually of a reddish tinge; sometimes into a reddish sandstone, subordinate to which are beds of incoherent shale. In North Devon they are very rarely so calcareous as to be burnt for lime, but in South Devon, rocks of the same age appear to be much more calcareous.

2. A series of rocks characterized by masses of hard thick-bedded red sandstone, and red micaceous flagstone, subordinate to which are bands of red, purple, and variegated shales. The red colour occasionally disappears, and the formation puts on the ordinary appearance of a coarse, silicious grauwacke, subordinate to which are some bands of imperfect roofing slate. In this series are very few organic remains. It is several feet in thickness, occupying the whole coast from the west end of the Valley of Rocks to Combe Martin.

3. The calcareous slates of Combe Martin and Ilfracombe, of very great aggregate thickness, abounding in organic remains, and containing in a part of their range at least nine distinct ribs of limestone burnt for use. This limestone is prolonged into Somersetshire, and appears to be the equivalent of that on the flanks of the Quantock Hills.

4. A formation of greenish and lead-coloured roofing slate of great thickness, and occupying a well-defined zone in North Devon, its upper bed alternating with and gradually passing into a great deposit of sandstones of various colours and micaceous flagstones. These silicious masses alternate with incoherent slates, and are in some places surmounted by great masses of red unctuous shale, which, when in a more solid form, generally exhibit cleavage oblique to the stratification.

5. The Silurian system resting conformably on the preceding, and of great thickness, on the north-western coast, containing many subordinate beds and masses of limestone. In its range towards the eastern part of the county it gradually thins off, but its characters are well preserved, and it everywhere contains vast numbers of characteristic organic remains.

6. The carbonaceous system of Devonshire, in a direction east and west across the county, in its southern boundary so close to Dartmoor that its lower beds have been tilted up and altered by the granite. It occupies a trough, the northern border of which rests, partly in a conformable position upon the Silurian system, and partly upon older rocks, probably of the division No. 4. Its southern border also rests on the slate rocks of Launceston. It everywhere exhibits a succession of violent contortions. In some places it is overlaid by patches of green sand, and west of Bideford by conglomerates of the new red sandstone. The lowest portion of this vast deposit is generally thin bedded, sometimes composed of sandstone and shale, with impressions of plants, sometimes of indurated compact slate, containing wavellite. These beds are surmounted by alternations of shale and dark-coloured limestone with a few fossils. Subordinate to these, there are on the western side of the county thin veins and flakes of culm or anthracite; but this is wanting on the eastern side, and the calcareous beds are more expanded. The higher beds of this deposit are well exhibited on the coast west of Bideford. These often contain impressions of vegetables. Though in a state of greater induration than the ordinary coal-measures of England, and even in many places destitute of any trace of coal, still these beds do not differ from the great unproductive coal-field of Pembrokeshire. They consequently concluded, that from the order of superposition,—from mineral structure—from absence of slaty cleavage peculiar to the older rocks on which this deposit rests, and from the specific character of its organic remains, it may without hesitation be referred to the regular carboniferous series.

In the course of the details, the authors alluded to a remarkable elevated beach, occupying two miles of coast on the north side of Barnstaple Bay, a more special account of which is being prepared for the Geological Society.

Mr. De la Beche objected to the conclusions of Messrs. Sedgwick and Murchison, although he did not dispute the correctness of the section of the country which they had exhibited to the meeting. He conceived that he had traced the carbonaceous rocks passing into what had been termed the Cambrian system, although he was not prepared to say that it really was that system. He was also unable to make that separation of the contorted rocks, suggested by the authors of the paper. He spoke of the overlying greenstones in different places, and considered that these were of different ages; also of the changes produced by granite on rocks of every kind in contact with it. He alluded to the former opinions of the rocks called by the general name, Greywacke, which opinions have, of late years, been totally altered. He attached very little importance to mineral characters; unless the consideration of the imbedded organic remains was made of the first importance, we were sure of falling into error. Are the organic remains in these carbonaceous rocks of Devon really the same as those of the general carboniferous system? He stated, that he conceived there was evidence to prove that there was a regular band of rocks surrounding Dartmoor, which had been thrust up through the hollow in the middle. He could nowhere discover any line of separation between the carbonaceous and the older rocks, so that he was unable to reconcile the deposits of coal with those of other parts of England, and with the age of these older rocks all were agreed. In the Alps, organic remains of the coal formation are found in beds, alternating with oolites, so that we must not limit too strictly the range of these organic remains, as we should be certain of all the conditions under which coal plants can be accumulated. We should recollect, that the remains of the vegetation of a mountain may be entombed at its base, so as to be shifted from its original habitat; and that, although the disposition of organic remains may hold true for a certain extent of the earth's surface, we have no right to consider such a disposition universal.—Mr. Sedgwick remarked, that he could with certainty distinguish four calcareous strata in North Devon—viz. one at Linton, a second at Ilfracombe, and two others at Barnstaple. The difference of the limestones of South Devon was also very remarkable; that of Plymouth being essentially distinct from that of Dartmoor. These carbonaceous strata also extended several miles into Cornwall.—Mr. Conybeare considered that the public had exaggerated the difference of opinion then before the meeting. He was rather inclined to coincide with Messrs. Sedgwick and Murchison in considering the strata in dispute as referable to the general carboniferous system, and from the general resemblance of the formations of those of Pembrokeshire, the probability was much strengthened.—Professor Phillips conceived that it had been satisfactorily proved, that there existed a coal basin in the interior of Devonshire, although, at first sight, from the unprofitable nature of the contained coal, being the kind called Culm, some hesitation might have taken place as to assigning it its true position. But doubts must vanish on inspecting the organic remains: and here he might observe, that it was a mistake to suppose that Dr. Smith, the founder of English Geology, had ever intended to limit the range of these remains as some had accused him of. We might readily assume, and observation has confirmed, that some organic remains of one stratum may be found in contiguous strata, associated with fossils of different kinds, so that organic remains alone are insufficient to point out distinctions in strata. But, the general appearance of the limestones of Devon was precisely similar to those of the north of England, in regard both of mineral character and imbedded fossils. From their appearance, he had expected their interstratification with shales, and Mr. Murchison had confirmed this supposition. The Devon limestone corresponded indeed with the upper bed of the Yorkshire limestone; in the former he had detected a shell, a species of Anodon, which he had not observed in the latter: but the species of Posidonia found in both exactly correspond. Perhaps one cause of mistake might have been the little attention paid to the black limestone of Craven, by Mr. Conybeare, and to this limestone there was a most striking resemblance in the black variety of Devonshire. He alluded to the extraordinary anomaly of coal plants having been found in the Alps, associated with oolites, but this might be an exception from the general law, and exceptions there must be; still it must be allowed, that organic life must have a constant relation to the state of the actual surface. He came to the conclusion, that the Devon district would not offer any anomaly in geological arrangement, but that it would correspond in arrangement with the other parts of the country, and that a fruit-

ful source of error is the hitherto vague term Greywacke, which has been applied indiscriminately to a great variety of rocks, so as to include many of different ages throughout this county.—Dr. Buckland congratulated the meeting on the difference of opinion among the geologists present, such a difference producing discussion, which was the sure means of arriving at truth. He considered, that the true solution of the question at issue would be in the middle course; that, no doubt, it could not be easily granted, that the series under consideration was carboniferous, when no true coal was contained in it; but, were we to adopt the new term, *culmiferous*, we should get rid of the difficulty. This culmiferous series he regarded as the lowest portion of the coal formation, and as resting upon the Silurian rocks. He alluded to the difficulty of making geological maps; these must be constantly modified, according to the extent of investigation: errors of omission must be committed by every pioneer in Geology, which can only be corrected by the researches of succeeding observers.

After the discussion was closed, Mr. De la Beche rose to submit some considerations on the connexion of the Geological Phenomena of Cornwall and Devon with the Mineral Veins of those counties. He commenced with defining the local Cornish terms of *lode*, *cross course*, *counter*, and *elvan*, the first three referring to veins of metallic, the latter to a vein of granitic matter. A number of these had been laid down in his geological map of Cornwall with the most perfect mathematical accuracy. He next mentioned the overlying masses of greenstone, which are of different ages, and in some are imbedded portions of the slate rocks. Granite must have been protruded last of all the rocks, as it cuts off the greenstone in many places; but the phenomena of the veins are still more singular, as first the elvans, and then the lodes, cut through everything. He referred to certain faults in the greensand district of Blackdown, and, most singular to relate, these faults exactly corresponded in direction with those of Cornwall, although the latter were highly metalliferous veins, while the former were fissures destitute of any valuable content. He therefore laid out the conditions of a profitable metalliferous vein, as deduced from the experience of practical miners, that it should be near the granite, and that the best signs were an elvan and a cross course. In the parish of St. Just these phenomena are in the highest degree remarkable; and, near Penzance, where the elvan courses are traversed, metals are sure to occur. The lodes of slate rocks are generally unproductive. Mr. De la Beche was particularly anxious to impress these facts on the public in general, as mining speculations had been of late so much the rage, that the more theoretical knowledge that could be diffused the better, so as to cause inquiry to be made respecting the geology of the mining district about to be entered upon by a joint stock company, before capital was invested in a hazardous, and perhaps ruinous enterprise.

Mr. Hopkins was called upon to make some observations regarding the direction of the fissures mentioned by Mr. De la Beche, but he did not enter very fully into any discussion, as he proposed, on the following day, to bring the general consideration of fissures before the Section. He observed, however, that there must have been one great axis of disturbance, to which the smaller fissures must either have been parallel, or have circulated around it—indeed, Mr. De la Beche had supposed the great line of fissures from Blackdown to Cornwall had been curved by the intervening granites. He stated, that there must be a connexion between the width of lodes and their mineral contents—also, that in the production of fissures there must have been several periods of elevation.

Mr. Fox then mentioned a remarkable experiment which he had made upon the yellow sulphuret of copper, having changed it by electricity into the grey sulphuret. In a trough a mass of clay was placed, so as to divide it into two portions, in one of which was sulphate of copper in solution, in the other dilute sulphuric acid. On the electric communication being made by placing the yellow sulphuret in the solution, and a piece of zinc in the acid, the change of sulphuret took place, and crystals of native copper were also formed upon it.—Mr. Taylor bore testimony to the importance of geological information to mining agents, who now were informing themselves, not only in practice, but in theory. He spoke of the exertions of the late Mr. Phillips, in drawing up a geological map of Cornwall, so far back as 1800. He suggested the propriety of tracing the lines of fissures into the coal districts, and also wished the directions of the lead lodes of the mountain limestone to be ascertained, as likely to lead to general results. —Messrs. Conybeare and Sedgwick made some observations on the importance of making use in Geology of such an agent as Electro-Magnetism.

Section D.—Zoology and Botany.

Professor Henslow in the chair.—Dr. Richardson resumed the reading of his Report on the Zoology of North America. In touching upon the geographical distribution of the mammalia, he remarked the great similarity which existed between them and the European species; whilst there was the greatest dissimilarity to those of South America. The boundary line separating the Faunas of North and South America, was not at the Isthmus of Darien, but at the tropic of Cancer. No Quadrumana occur to the north of the Isthmus of Darien; though in Europe there is a species which ranges as far north as the rock of Gibraltar, in latitude 36°.

In the order Carnivora, and family Cheiroptera, all the North American species belong to that tribe which possesses only one bony phalanx in the index, and two in each of the other fingers, to which tribe also all the European bats belong, except an Italian species of Dinops. None of the sixteen species recorded as natives of North America have been found elsewhere: two only have been traced over any great extent of country, and one of these (resembling the European Pipistrellus) ranges through 24° of latitude, and is the most northerly species in America. There must be still many bats to be discovered in that country, as those of Mexico, California, and the whole tract of the Rocky Mountains are entirely unknown.

Of the family Insectivora, ten species were enumerated; and it was stated that North America differs more from Europe in this family, than in any other of the order Carnivora. Three of the European genera do not exist in North America, and the three genera found in North America do not exist in South America. The North American species of Sorex, however, closely resembles those of Europe.

Of the family Marsupiata, inhabiting the New World, only three species reach into North America, the rest being confined to the south of the Isthmus of Darien. Two of these occur no higher than Mexico; but the third (the Virginian opossum) ranges to the great Canadian lakes on the north, and to Paraguay on the south.

About forty species of the family Carnivora have been noticed; and this family includes a greater number than any other which are common to both North America and Europe; though possibly a closer acquaintance with some which are at present considered identical, may enable us to discriminate between them. The generic forms of North America are the same as those of Europe, excepting in a very few cases, which belong to the South American group. A few of the more northern forms also cross the Isthmus of Darien to the south.

In the family Plantigrada, two of the four bears of North America are undoubtedly peculiar to the New World; and one of these is the most northerly quadruped it contains. The American Glutton, or Wolverine, according to Cuvier, is identical with that of the Old World. Among the Digitigrada, the range of the Mustelæ is limited southwards to the northern or middle districts of the United States. Whether any of the American and European species of this genus be really identical, is involved in great uncertainty. Of the three Otters of North America, one appears to be identical with that of Europe; and another, if correctly identified as the *Lutra Brasiliensis*, has a most extensive range, from the Arctic Sea through great part of South America.—Eight species of the genus Canis are found in North America; but there is great difficulty in distinguishing the species, and in identifying them with any of those of Europe. The domestic dog breeds with the wolf and fox, and their offspring is prolific.

Eight species of the genus Felis were mentioned by Dr. Richardson, three of which extend from South America into the south-western territories of the United States; and some of the others are still doubtful as North American species.

The nine species of Amphibia found in North America, are mostly common to the northern seas of the Old and New Worlds: the genus Otaria alone being confined to the North Pacific; and even these range to the Asiatic coast. The specific identity of some of the seals is involved in very great doubt.

In the order Rodentia, there have been between seventy and eighty species discovered; and here North America surpasses every quarter of the globe in the abundance and variety of form which these animals assume. The squirrels are not yet satisfactorily determined. The marmots are numerous, excepting in the sub-genus Spermophilus. There is only one which may possibly be common to the New and Old World. There is only one of the restricted genus Mus, which is unequivocally indigenous to North America; and this closely resembles the European *M. sylvestris*. Other species have been introduced from the opposite side of the Atlantic.

Mr. Bowman read a communication respecting the Longevity of the Yew-Tree; and mentioned the result of his observations upon the growth of several young trees, by which it appeared that their diameters increased, during the first 120 years, at the rate of at least 2 lines, or the one-sixth of an inch per annum; and that under favourable circumstances the growth was still more rapid. In the churchyard at Gresford, near Wrexham, North Wales, are eighteen yew-trees, which are stated by the parish register for 1726 to have been planted in that year. The average of the diameters of these trees is 20 inches. Mr. Bowman then remarked on two yew-trees of large dimensions, from the trunks of which he had obtained sections. One is in the same churchyard as those above mentioned, and its trunk is 22 feet in circumference at the base, 29 feet below the first branches. This gives us a mean diameter of 1,224 lines, which, according to De Candolle's rule for estimating the age of the yew, ought also to indicate the number of years. From three sections obtained from this tree, Mr. Bowman ascertained that the average number of rings deposited for one inch in depth of its latest growth, was 34½. Comparing this with the data obtained from the eighteen young trees, he estimated the probable age of this tree at 1,419 years. The second of these trees is in the churchyard of Darley in the Dale, Derbyshire, and its mean diameter, taken from measurements at four different places, is 1,356 lines. Horizontal sections from its north and south sides gave an average for its latest increase at 44 rings per inch nearly, which gives 2,006 years as its age, by the mode of calculation adopted by Mr. Bowman. He then proceeded to state his opinion of the reasons why so many old yew-trees were to be met with in churchyards: he considered that might have been planted there at a period anterior to the introduction of Christianity, under the influence of the same feelings as those which prompted the early nations of antiquity to plant the cypress round the graves of their deceased friends.

Mr. Ball exhibited the skulls of a species of seal common in Ireland, with the view of eliciting information, as he considered it to be new to the British Fauna, and very distinct from the two already recorded. The present species was never known to become tame, whilst the *Phoca vitulina*, generally considered the more common species of our coasts, was very easily tamed.—Prof. Nilsson, of Leind, at once pronounced this species to be his *Haliochœrus griseus*, forming a distinct genus from *Phoca*, and described by him in the year 1820. It had been previously recorded by Fabricius, under the name of *Phoca gryphus*. It is common in the Baltic and North Sea, and to be met with in Iceland, and attained to a size of eight feet in length. In Sweden it was emphatically termed the Sea-seal, in contradistinction to those which inhabited gulfs. He remarked that the name of *Phoca vitulina* had been applied by Linnæus, and subsequent authors, to three distinct species, to which he had himself given the names of *barbata*, *variegata*, and *annellata*. Of these he had ascertained that a specimen, captured in the Severn, and now in the Bristol Institution, belonged to the *annellata*.—Dr. Scouler remarked that the

species which Prof. Nilsson had identified as his *Haliochœrus griseus*, predominated in Ireland over the *Phoca vitulina*, though it had been hitherto neglected; and that the great difference in the teeth of these species, justly entitled them to be considered as forming distinct genera.—Dr. Riley exhibited the stomach of the specimen alluded to, as having been captured in the Severn, in which he had found from thirty to forty pebbles, and states that other instances had occurred of a similar nature; and that it was a popular notion that they assisted the seal in the way of ballast whilst catching his prey, which he did by rising vertically upwards, and seizing it from below. But Sir Francis Mackenzie then asserted that he had repeatedly seen the seal chase salmon into the nets, and that it was not usual for it to capture its prey in the way described. Neither he, nor Professor Nilsson, nor Mr. Ball, had ever found stones in the stomach of this animal.

Dr. Hancock read a paper on a new species of Norantea, from Guiana, termed by the natives Corocoromibi. This grows on the banks of rivers, and in moist places, and its botanical characters closely resembling those of the *Norantea Guianensis*; he had long confounded it with that species. As Aublet's plant is, however, described as a tree which grows eighty feet in height, and as the present species is a large climber, they must be distinct; and Dr. Hancock then detailed the botanical characters of the latter.

Mr. Hope exhibited a remarkable specimen of the *Lucanus camelus*, Fabr., from North America, the right side of which had the configuration of the male, and the left of the female sex. This monstrosity was analogous to one which had been observed in the *Lucanus cervus*, a closely allied species of Europe. The exhibition of this specimen led to a discussion, in which Mr. Curtis, Dr. Riley, and Mr. Yarrell took part, concerning those principles of developement, by which monstrosities of the above description were reducible to the operation of general laws. Mr. Yarrell particularly noticed the occurrence of both male and female organs, on opposite sides of various hermaphrodites, in lobsters, and birds, which he had dissected, and stated that he had met with an instance of a fish, which had a hard roe on one side, and a soft one on the other. He had met with a very extraordinary example of double sex in a fowl, which he had not yet made public, and of which he now detailed some of the more interesting particulars.

Mr. Hope read a communication, expressive of the probability that some of the early notions of antiquity were derived from the observation of insects. In attempting to account for the apparently spontaneous generation of those insects, which rise in myriads from the mud left by the waters of the Nile, the philosophers of antiquity turned their earliest attention to the operation of the external influence of the elements, and Mr. Hope, supporting his opinion by numerous quotations, showed that they considered the sun as the chief and efficacious power in producing this effect. The opinion of spontaneous generation was universally adopted, and in full force till the middle of the sixteenth century, and is still retained in the greater part of Asia and Africa, and even held by certain eminent naturalists in Europe. The origin of the doctrine of a metempsychosis, he considered, might be deduced from their actual observation of the metamorphosis of certain insects. This doctrine is now confined to the Gawrs of Persia, and some other idolatrous nations of the Asiatic continent.

Mr. P. Duncan offered a few remarks upon the subject of Mr. Hope's speculations.

Mr. G. Webb Hall commented on the effects of lime as variously applied to different soils, and considered the general effects of this substance, with respect to its value as calcareous earth, and its septic qualities as facilitating the decay of vegetable matter. In the latter capacity it was found to be most beneficial in a humid climate like that of Devonshire. He pointed to the necessity of a scientific inquiry for the purpose of obtaining more precise information than we yet possessed, as to the requisite proportions in which lime should be furnished to land of different qualities. He had found that less was required, and a greater benefit produced, by employing lime fresh from the kiln, and ploughing it into the ground within twelve hours of its being laid on the surface. He bore testimony to the value of gypsum as a manure for lucerne. Mr. Rootsey was sceptical as to the ill effects so universally attributed to magnesian limestone, and which had been alluded to by Mr. Hall, as he knew an instance where very large crops were obtained from a district, where this rock prevailed.

Section E.—Anatomy and Medicine.

Dr. Roget in the chair.—The first paper read was entitled, 'Observations on Remedies for Diseases of the Brain,' by Dr. Prichard, of Bristol.—Dr. Prichard remarked, that perhaps all curative attempts in cases of disease affecting the brain resolve themselves into the modifications which medical art is capable of effecting in the vascular state, of parts within the skull. We can promote by various means either fulness or inanition of the blood-vessels in the brain: whether anything beyond this is in our power, is very uncertain. Besides general and local bleeding, all those means belong to the same class, which act by refrigerating or heating the surfaces either of the head or of other parts. Refrigerant applications to the head have the effect of contracting the calibre of the arteries, and thereby diminishing the quantity of their contents. Pediluvia, or other means of applying warmth to the lower extremities, produce a similar result by augmenting the capacity of vessels remote from the head, and causing a greater quantity of blood to be determined into them. All these means plainly owe their efficacy to the modification which they bring about in the state of the vascular system of the brain. The only class of remedies respecting the *modus operandi* of which any question can be raised, are those which produce what is termed counter-irritation; and perhaps the doubt which exists in this instance arises from the obscurity of the subject. It is very generally supposed, and perhaps correctly—at least it is very difficult to find any other hypothesis on the subject that is more probable—that the means of counter-irritation, such as rubefacients, vesicatories, and issues, produce their effect by lessening an hypoplethoric state of the vessels in internal parts, and that they bring this to pass by increasing the fulness of the vessels in surfaces to which they are immediately applied. There are facts which it is very difficult to reduce under this sort of explanation; as, for example, the relief obtained in cases of pneumonia or of bronchitis, by means of blisters applied to the parity of the chest, there being in these instances no continuity of structure that might render the proposed explanation in some degree intelligible. On the other hand, there is little doubt that such remedies are most efficacious when they are applied over surfaces nearly in juxtaposition with the seat of disease; and this fact, if not called in question, goes far towards establishing the notion before alluded to as to their mode of operation.

A case has lately occurred in my practice at the Bristol Infirmary, which strongly exemplifies the efficacy of the treatment which I have recommended, and which I have fortunately an opportunity of bringing before the Medical Section in the most convincing way. A youth, aged about eighteen, came into the Infirmary, labouring under complete amaurosis, which had been coming on gradually for a week or ten days before his admission. At that time it had become so complete, that vision was entirely lost, and the pupils were totally insensible to light even when the rays of the sun were suffered to fall immediately into the open eyes. At first he was freely and repeatedly bled from the arm and temporal artery, had leeches applied to the scalp, blisters to the nape of the neck, and took calomel so as to render his gums sore. Finding that no effect whatever was produced by these measures, I gave up the expectation which I had at first entertained of his recovering sight, but was resolved to give the remedies a complete trial. I ordered him to be bled *ad deliquium*. This took place after a small quantity of blood had flowed from his arm while he was in an erect posture. After a few days (he was still perfectly dark) an incision was made over the sagittal suture from the forehead to the occiput. It was filled with peas. In three or four days, precisely at the time when suppuration began to take place, the patient declared that he perceived light, but was scarcely believed, since the pupils were still widely dilated and quite insensible to a strong light. In the course of a few days it was quite evident that he saw—he could tell when two or three fingers were held up. For some weeks the iris was still quite irritable, though vision had become in a great degree restored.

The subsequent treatment of the case consisted chiefly in occasional leechings, purging, and low diet. When the issue healed, which was not till it had been kept open for some months, a seton in the neck was substituted: under this treatment the case has terminated in a complete recovery of the blessings of sight.

Dr. O'Beirne stated, that the treatment was perfectly new to him, and he should feel himself amply compensated if he derived no other benefit than hearing this paper from his attendance at the British Association.—Dr. Carson stated, rather as an objection, that if an animal were bled to death, the same quantity of blood would be found in the cranium, and that the doctrine of determination of blood to the head was unfounded, and frequently led to great errors in practice.

The second paper read was by Dr. Houston, on a human fœtus without heart or lungs. Several drawings were exhibited, and the reading of the paper led to a short discussion, in which Drs. Prichard, Carson, O'Beirne, Macartney, and Mr. Carmichael, took part.

The third paper was by R. Carmichael, Esq., on Tubercles.—Mr. Carmichael commenced with some remarks on the great prevalence of these formations, and then proceeded to detail their appearances according to Laennec and Carswell. He adverted to the use of the term Scrofula, which he considered a cloak for ignorance; and, having stated that Drs. Todd, Clark, and Carswell, believe in the identity of Scrofula and Tubercle, disputed this position, and likewise their opinion, that tubercles are inorganizable deposits. Among other objections he urged the inconsistency of representing enlarged cervical glands and pulmonary tubercles as identical, since it is well known that the former may be injected, but not the latter; and of maintaining the non-inflammatory origin of tubercles, together with the view, that these bodies are lifeless matter; since, if such is their nature, they must excite inflammation in the tissues which contain them. He allowed, however, that the scrofulous constitution disposes to tubercles, but only in the same manner as to cancer.

Mr. Carmichael next adverted to the generally-recognized connexion between Scrofula and disordered digestion, and claimed the priority of this observation by reference to a work which he published in 1810. He then proceeded to argue, at considerable length, in favour of the parasitical origin of tubercles, pointed out the absence of vascular communication between these bodies and surrounding parts, and observed, so long as the former retained their vitality, no inflammation takes place. The author declared his opinion, that Carcinoma must likewise be arranged among the Entozoa; and, having indicated the division of a cancerous formation into a medullary and a cartilaginous portion, assigned to the former an independent vitality, the latter being only a barrier which nature sets up against the parasite, and observed, that the containing cyst belongs to the surrounding tissue. The cartilaginous portion he stated might be injected, but not so the medullary tubercles, which he considered more allied to Carcinoma than to Scrofula. Having spoken of a difference between Fungus Medullaris and Fungus Hæmatodes, he proposed to arrange the formations which had passed under review, as constituting four species of Entozoa:—1. Tubercles found in the lungs. 2. Tubercles found in the abdominal organs. 3. Fungus Medullaris and Fungus Hæmatodes. 4. Carcinoma.

Mr. Carmichael next considered the exciting causes of tubercles, and concluded by urging, that practitioners must direct their attention rather to the prevention than the cure of the disease.

A short discussion followed. Some objections were brought forward by Dr. Macartney, and answered by Mr. Carmichael.

Section F.—Statistics.

Mr. Kingsley presented and described several forms of tables, for more accurately displaying the revenue and expenditure of the United Kingdom, and procuring accuracy in Parliamentary Returns of the state of Savings Banks, &c.

Dr. Bowring observed, that those who complained of the imperfection of parliamentary returns, forgot that they were ordered, not for the service of general science, but to serve some special purpose, or as the foundation of some particular motion. The means for procuring an accurate return of the revenue did not exist in this country. In most continental nations the whole gross amount of revenue is paid directly into the Exchequer; but in England, several departments arrest the amount necessary to pay their own expenses *in transitu*, and the number of these departments renders the accounts of British finance very complicated.—Mr. Tiarcks said, that measures had been taken to remedy the abuses from the former mode of superintending savings banks. Weekly returns were now made to the National Debt Office of the most minute description. In the savings bank of Moorfields, the deposits amounted to 520,000*l.*, every penny of which was accounted for weekly.

Baron Dupin addressed the Section on the subject of a paper he had laid upon the table, entitled, 'Researches relative to the Price of Grain, and its influence on the French Population.' He had extended his survey over a space of fifteen years, from 1815 to 1832, but had stopped at the latter year, in consequence of the special derangement produced by the cholera. During this interval, the price of corn in France had varied from 80*s.* to 34*s.* per quarter, and he proposed to examine the effect of this enormous disproportion on the elements of social life,—deaths, births, and marriages. From a variety of tables it appeared, that a difference of more than 100 per cent. in the price of corn produced an incomparably less variation in mortality than other causes which are unperceived and unappreciable; and that the effect of scarcities in the nineteenth century on mortality must be reduced to the rank of secondary causes, which can only be evolved by the artifices of calculation, and by grouping together a great number of years. The effect on births is scarcely greater—an increase of 50 per cent. on the price of grain produced only a diminution of $\frac{13}{1000}$ in the births. On marriages the effect was rather more marked: there were 918 less marriages for every million during the year of greatest scarcity than there were during the year of abundance: but the years of greatest abundance were not the years of most marriages, nor of the greatest social happiness. Society was most prosperous when provisions were at an intermediate price. The small annual variation in births, deaths, and marriages, even for years of great difference of price, induced the Baron to search for *a function* of these three social elements, which would both render the variations more perceptible, and, correcting one by the other, would remove the perturbations arising from accidental causes. This function is the mean between the number of births divided by the number of deaths, and the number of marriages divided by the number of deaths. It is sufficiently obvious, that this function is independent of the amount of population, and the Baron considered that its magnitude is a very fair test of social prosperity. He proposed to name it the Function of Vitality. In the years of extreme scarcity, the function of vitality averaged 0.5937. In the years of high prices it averaged 0.6092. In the years of intermediate prices it averaged 0.6168. —He then observed, that according to Dr. Cleland's paper, read on the preceding day, the function of vitality in Glasgow was about 0.7000—a clear proof that social happiness was greater in England than in France. He trusted that this function would be calculated for the principal continental nations and for different epochs, in order to compare their social prosperity by a precise and identical standard. As one valuable result, he showed that this function was far less in England during seasons of commercial depression than of agricultural distress.

In illustration of the Baron's views respecting the price of corn, and its relation to the function of vitality, Mr. Porter read the following table:—

	Price.	Baptisms.	Burials.	Marriages.
1801	115.11	237	294	67
1802	67.9	273	199	90
1803	57.1	294	203	94
1810	103.2	298	208	84
1812	122.8	300	190	82
1815	63.8	344	197	99
1822	45.3	370	220	98

Lord Nugent remarked, that while the population of France was notoriously increasing, the number of births appeared to be stationary. Baron Dupin said, that this was owing to the progress of civilization; fewer children, comparatively, were born, and fewer died.

Mr. Porter read the following table, in confirmation of the Baron's views:—

LONDON BILLS OF MORTALITY.

Deaths under 20 years of Age.

1751—60.....	51⅞ per cent.	
1761—70.....	50⅛ —	Difference between first and last of these decennial periods, 1¾ per cent.
1771—80.....	52¼ —	
1781—90.....	49⅞ —	
1791—1800....	49⅞ —	
1801—10.....	47⅛ —	Difference between 1751—60 and the last of these decennary periods, 7¾ per cent.
1811—20.....	45⅜ —	
1821—30.....	46⅛ —	
1831—33.....	42⅜ —	

Deaths in Christ's Hospital.

1814—18	1 in 100
1819—23	1 — 128
1824—28	1 — 135
1829—33	1 — 157½

	Births.	Marriages.	Deaths.
1697—1800.....	1 in 36.....	1 in 48.....	1 in 123
1806—1820.....	1 — 32.....	1 — 49.....	1 — 121
1826—1830.....	1 — 34.....	1 — 51.....	1 — 128

Some inquiry was made respecting the effects of Vaccination, to which Mr. Porter replied by producing the following table:—

Persons dying of Small Pox within the London Bills of Mortality.

1750—1760.....	94 in 1000
1770—1780.....	102 —
1810—1820.....	43 —
1820—1830.....	35 —
1831—1833.....	23 —

Mr. Fripp thought it a great anomaly that a low price of corn should not, of necessity, produce a high function of vitality, and he attributed this to a want of forethought in the labouring population, owing to their deficiency of education.—Dr. W. C. Taylor said, that the anomaly was explicable on elementary principles. A very slight excess of supply above demand produced a very disproportionate fall in price. He instanced two or three cases in which the disproportion had been very great. Baron Dupin had noticed the great disproportion between the fall in price and increase of supply, both in the essay before the Section, and in his memoir on National Industry, addressed to the Institute in 1831. Dr. Taylor was of opinion that this was precisely the case in which Statistics failed us, for whether the excess of supply over demand was small or great, of course within certain limits, the fall in price was sure to be excessive.—Mr. Harman Visger supported Dr. Taylor's views.—Colonel Sykes and Dr. Turner stated, that from the result of their inquiries it appeared evident that human happiness always increased with the cheapness of the necessaries of life.—Baron Dupin observed, that when he said the greatest social happiness appeared at the intermediate price of corn, he did not intend the mean between the highest and the lowest, but a price a little above the lowest.—After some desultory conversation, the Delegates from the Asiatic Society were called upon to state the subject of their mission.

Col. Sykes said, that a proposal had been laid before the Asiatic Society by the Right Hon. Holt Mackenzie and Dr. Royle, for establishing a committee, with affiliated branches, to collect statistical information with respect to the natural and artificial productions and wants of India. The project has been already explained in the report of the Asiatic Society, published in this journal.‡ Col. Sykes read several extracts from the communications made to the Asiatic Society, by Mr. Mackenzie and Dr. Royle, showing that the resources of India were yet comparatively undeveloped, and that a vast supply of materials for manufacture might be derived from that country. He dwelt particularly on cotton, for which we were now principally dependent on America.—Dr. Taylor observed, that our empire over India was completely the supremacy of knowledge. He entered at considerable length into the question of the trade between India and the ports on the Levant and Euxine, which he stated to be constantly and rapidly increasing; and detailed some particulars of the markets of Cabul, Bokhara, and Herat, obtained from recent publications of the Calcutta government. He instanced, as a proof of the benefit that would result from the proposed series of inquiries, the advantages which British commerce had derived from the information collected by recent travellers in the East.—Mr. Porter confirmed this statement, and directed attention to the rapid increase of trade between London and Trebezond.—Mr. Visger stated, that he was engaged in a branch of manufacture which depended on the supply of lichens; and he could assure the Section that this branch of industry, almost entirely unknown, was rapidly rising, both in extent and importance. Lichens were imported to the amount of 100,000*l.* per annum; but the supply of the more valuable sorts was already beginning to fail, and gentlemen acquainted with botanical science had been engaged to travel in search of them. Should this part of the subject engage the attention of the proposed Committee, he would gladly supply specimens of lichens, with descriptions, to the Asiatic Society; for he felt assured, from all the accounts he had read, that India was likely to be the habitat of some of the most valuable plants of the lichen tribe.—Dr. Robison, of Edinburgh, said, that there were two products of India likely, when sufficiently known, to become valuable articles of commerce: he meant a pulp for the manufacture of paper, obtained from Nepaul, the paper derived from which was the only one that resisted the action of worms; and a wood-oil which, from its durability and fragrance was particularly worthy the attention of house-painters.—Lord Sandon, after some allusions to our ignorance of Indian statistics, dwelt very strongly on the necessity of attending to the cultivation of cotton in India. The stock of raw cotton on the manufacture of which so large a portion of our population depended for subsistence, was often in a very precarious state. Of his own knowledge, there was a period, about eighteen months ago, when the stock on hand would not have supplied six weeks' consumption. We now depend on the United States of America for supply. He would not refer to the possibility of war with that power. God forbid that two nations, bound together by identity of origin, interest, and civilizing influence, whose bonds of amity were the strongest this world ever witnessed, should again engage in what was almost civil war. But he need not remind the Section that the Southern States, from which our supply of cotton is derived, are in a very ticklish state, owing to the existence of slavery, and the question of negro emancipation, which is very likely to precipitate some sudden conclusion of no very peaceful nature by the Texian war.—Several gentlemen from the manufacturing districts professed their anxiety to aid the views of the Asiatic Society, in establishing the proposed Committee.

‡ See Athenæum, No. 447.

Section G.—MECHANICAL SCIENCE.

The sitting of the section occupied but a short time, during which two papers were read, one of some interest, by Mr. Henwood, on Naval Architecture, and a second by Mr. Coosham on certain improvements in Napier's rods. Dr. Daubeny also exhibited an ingenious instrument for taking up sea water from any given depth, for the purpose of chemical analysis, being an improvement of an admirable invention for that purpose sent out in the *Bonite*.

EVENING MEETINGS.

In consequence of the incessant rain, the intended promenade and Horticultural exhibition at Miller's Gardens was abandoned, and notice given that the Geological, Statistical, and Mechanical Sections would meet in the evening.

In the Geological Section, Dr. Hare, of Philadelphia, entered upon a history of the many modifications of the Pile of Volta, and in particular drew attention to a form of it, devised, and long since described by himself, but which he conceived had not in a sufficient degree attracted the attention of European philosophers. His apparatus is compact, portable, and, what is a capital advantage, admits in an instant, and by the simplest manipulation, of being put in action, and having this action suspended. A prodigious quantity of acid is thus saved, which would otherwise go to waste, and the operator is enabled to avail himself, as often as he chooses, of that superior influence which is so well known to be manifested by the pile at the first instant of its excitation. Dr. Hare concluded by the exhibition

of some striking experiments illustrative of the igniting or deflagrating efficacy of his Voltaic arrangements.

Prof. Phillips followed with an account of the distribution over the northern parts of England of Blocks or Boulders. The Association, he observed, had formerly proposed a question regarding this distribution, and the present was a partial attempt at its solution; and it was interesting both to the geologist and the geographer, as it involved the effects of running water in modifying the surface of a country. In glancing over the North of England, we find a great variety of rock formations, from the oldest slates to the newer tertiary; the country generally slopes to the east, with the exception of the group of Cumbrian mountains, which form a local conical zone. One striking feature in its physical geography, is an immense valley running north and south, and passing through a great variety of formations; the Wolds of York being chalk, of Whitby oolite, the vale of York new red sandstone, while the carboniferous rocks are displayed in Northumberland and Durham. All the country from the Tyne to the Humber is covered with transported boulders, many of which are of rocks quite different from any near the spots where they occur, and some even not recognizable as British rocks. Could Mr. Lyell's ideas regarding the office of icebergs be true, that they had been the means of transporting gravel to distant places? Boulders of the Shap Fell granite had been found in the south-eastern part of Yorkshire; in the interior, there were great accumulations of them in many places; their directions seemed all to converge to a certain point, in what is termed the Pennine chain, but on this chain no boulders have been observed, except at one point, from which you look towards Shap Fell; towards the north they have been drifted nearly as far as Carlisle, but there is no trace of them towards the west. We also may find boulders fram Carrick Fell carried to Newcastle and the Yorkshire coast, and these have been drifted over the same point of Stainmoor. Mr. Phillips gave several conflicting opinions of different geologists, to account for this extraordinary transportation: the bursting of the banks of lakes; the alternate elevation and depression of mountain chains; and the supposition that the entire country had been under the sea, when the distribution of boulders had taken place.—Mr. Sedgwick then rose, and remarked, that the direction of transport of the blocks may have been modified by the surface over which they were carried; and that Sir James Hall had been the first who had observed the Shap Fell boulders. These boulders Mr. Sedgwick had noticed on the shores of the Solway Frith, mixed with gravel from Dumfriesshire. He alluded to the action of water upon the crests of mountains, and to the occurrence of transported blocks at considerable elevations. It was well known that mountain lakes were gradually filling up; and he had shown in a paper to the Geological Society the relation of a lake to the age of the valley containing it. With the diluvial gravel over the country we find associated organic remains,—a strong proof that the land must have been dry when the transportation took place.—Mr. Murchison had observed these boulders associated with recent shells at various elevations,—consequently, the land must have been at one time under the sea, and have been subsequently elevated. There must have been a relative change of the level of land and sea; and Professor Esmark, in Norway, had been the originator of the idea of the icebergs transporting gravel. He referred to the valley of the Inn, in the Tyrolese Alps, as illustrating this alteration of level: boulders of granite had been found on calcareous mountains composing one of its sides, elevated five or six thousand feet above the sea level; and this valley could not have been scooped out.—Dr. Buckland was of opinion that the land must have been dry before the action of the water that had transported these blocks. There was a great number of organic remains mixed with the gravel, derived from animals existing on dry land; and this was not only true in England, but confirmed by observations in the continent of Europe.

In the Statistical Section, Dr. Lardner delivered a lecture on Steam Communication with India—a subject on which we have dilated too recently, and at too great length, to return to it in the present crowded state of our columns.

In the Section of Mechanical Science, Mr. Whewell gave a short account of the present state of the science of the Tides. Though there can be no doubt, he observed, that the tides are to be reckoned among the results of the great law of universal gravitation, they differ from all the other results of that law in this respect, that the facts have not, *in their details*, been reduced to an accordance with the theory; and the peculiar interest of the subject at the present moment arises from this, that the researches now going on appear to be tending to an accordance of theory and observation; although much in the way of calculation and observation remains to be still effected before this accordance reaches its ultimate state of completeness. With regard to observation, the port of Bristol offers peculiar advantages; for in consequence of the great magnitude of the tides there, almost all the peculiarities of the phenomena are magnified, and may be studied as if under a microscope. With regard to the theory, one point mainly was dwelt upon. By the theory, the tides follow the moon's *southings* at a certain interval of time, (the *lunitidal* interval,) and this mean interval will undergo changes, so as to leave less than the mean when the moon passes three hours after the sun, equal to the mean when the moon passes six hours after the sun, and greater than the mean when the moon passes nine hours after the sun; and the quantity by which the lunitidal interval is less than the mean when the moon is three hours after the sun, is exactly equal to the quantity by which the lunitidal interval is greater than the mean when the moon passes nine hours after the sun. And this equality of the defect and excess of the interval at three hours and at nine hours of the moon's transit, is still true where the moon's force alters by the alteration of her parallax or declination. Now we are to inquire whether this equality of excess and defect of the interval in all changes of declination, &c. is exhibited by observation. It appears at first sight, that the equality does not exist; that is, if we obtain the lunitidal interval by comparing the tide with the *nearest* preceding transit. But, in truth, we ought not to refer the tide to such a transit, because we know that the tide of our shores must be produced in a great measure by the tide which revolves in the Southern Ocean, and which every half day sends off tides along the Atlantic. The tide, therefore, which reaches Bristol, is the result of a tide wave, which was produced by the action of the sun and moon at some anterior period. It is found, that if at Bristol we refer each tide to the transit of the moon, which took place about forty-four hours previously, we do obtain an accordance of the observations with theory in the feature above described, that although the moon's force alters by the alteration of her declination, the defect of the lunitidal interval for a three hours' transit of the moon is equal to the excess of that interval for a nine hours' transit. And thus, in this respect at least, the tide at Bristol agrees exactly with the tide which would be produced, if, forty-four hours before the tide, the waters of the ocean assumed the form of the spheroid of equilibrium due to the forces of the moon and sun, and if this tide were transmitted unaltered to Bristol in those forty-four hours.

OUR WEEKLY GOSSIP ON LITERATURE AND ART.

In devoting our entire columns to the Report of the proceedings of the British Association, we comply with what we believe to be the general wish of our Subscribers. No cost or exertion has been, or shall be, spared to render it full and satisfactory. We had hoped, by giving a double Number, to have brought it down to Wednesday night; but, though we have strictly confined ourselves to the scientific business of the meeting, the voluminous Report of the Secretaries, the abstract given of many papers, and the necessary diagrams which, with great exertions we have been enabled to prepare in illustration, have occupied so much space, that we find it impossible without injudicious abridgments. Next week, however, we will give another double Number—a third if necessary—and these will enable us to clear off arrears with publishers and advertisers, to whom we apologize for this unusual disregard of their interests and *our own*.

It may be well here to announce, for the information of the scientific public, that the meeting of the Association of German Naturalists, will take place on the 10th of September, at Jena, and will break up on the 26th of the same month.

We regret to have to announce the loss which science has sustained by the death of Mr. Edward Turner Bennett, Secretary to the Zoological Society, who died after a short and painful illness on Sunday, the 21st inst. From the affability of his manners, and general scientific and literary attainments, he was highly respected by a numerous circle of friends. He was author of several papers, published in the Transactions of the Zoological Society, and of two popular volumes on natural history, 'The Tower Menagerie' and 'Garden and Menagerie of the Zoological Society;' and he was about to publish a new edition of White's 'History of Selborne,' which would, no doubt, have proved acceptable to all admirers of that interesting work.

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On and after Saturday the 17th of September, the Stamped Edition of the ATHENÆUM will be reduced in price from *Eightpence* to *Fivepence*.

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MIDDLESEX HOSPITAL.—The WINTER SESSION will commence on the 1st of October, with an INTRODUCTORY ADDRESS, by Dr. Copland.
ANATOMY and PHYSIOLOGY—Mr. Tuson and Mr. Shaw.
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SUMMER SESSION, May 1, 1837.

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This Establishment will be opened in the latter end of August, for the EXHIBITION of PAINTINGS in OIL and WATER COLOURS, SPECIMENS of SCULPTURES and CASTS, ARCHITECTURAL DESIGNS, MODELS, and PROOF IMPRESSIONS of ENGRAVINGS, subject to the following

REGULATIONS.

I. All Works of Art sent for Exhibition or Sale must have the Names of the respective Artists conspicuously marked thereon, and if more than one, they must be numbered;—a List containing the number, a written description of the several performances, the price with or without the frame and glass, (if intended for sale,) together with the residence of the Proprietor or Artist, must be addressed to the Superintendent, G. A. Hillier, at No. 6, Royal Victoria Arcade, Ryde.

II. No Picture can be admitted without a frame.

III. A Commission of ten per cent. will be charged on the amount of all Works of Art sold, and all expenses to and from Artists to the Saloon of Arts will in consequence be paid by the Superintendent.

IV. All Monies arising from the sale of Works of Art, will be paid to the respective Proprietors, when received from the Purchasers.

V. Every care will be taken of the Works of Art, but the Superintendent does not hold himself responsible for any damage accidentally incurred.

VI. As the object of this Establishment will be the CONTINUED Exhibition of Works of Art, those intended for sale will be allowed to be removed as soon as sold; but those intended for Exhibition only, must remain for Three Months at least.

Mr. J. Green, jun. 14, Charles-street, Middlesex Hospital, will send for, pack, and forward to Ryde, Pictures which any Artist residing in London may be desirous of sending.

For further particulars, or to take Shops in the Royal Victoria Arcade, application to be made (if by letter, post paid,) to Mr. G. A. Hillier, No. 6, Royal Victoria Arcade, Ryde, Isle of Wight.

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